

The British Journal for the Philosophy of Science

VOLUME IX

AUGUST, 1958

No. 34

ON DIMENSIONALITY AND CONTINUITY OF PHYSICAL SPACE AND TIME *

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I *Introduction*

'Consider, for example, the words on this page. To a casual glance they form a two-dimensional distribution; but they were written in the hope that the reader would regard them as a one-dimensional distribution.'

A. Eddington, *Space, Time and Gravitation*

RECENTLY an article was published in this *Journal*, which investigated an interesting problem of the causes for three-dimensionality of space.¹ The author, however, left without consideration the question: does space actually have three dimensions? Although the affirmative answer to this question seems to be self-evident, it is worthwhile to subject the whole problem to a detailed analysis and to clear up the meaning of the statement that our physical space is three-dimensional and that the world, time included, is four-dimensional.

As was pointed out by Eddington² this 'is not so clear as it appears at first. An aggregate of a large number of things has in itself no particular number of dimensions. In order to define the number of dimensions we have to postulate some ordering relation. This relation appears to be the interval.' He demonstrated convincingly the importance of the conventional assumption of a particular ordering relation by an example, cited above, of dimensionality of the words printed on a page.

It will be the task of this article to try to detect also other conventions, which explicitly or implicitly were made, in defining the number of dimensions of our physical space and time.

* Received 28. vi. 57.

¹ G. J. Whitrow, this *Journal*, 1955, 6, 13

² A. Eddington, *Space, Time and Gravitation*, Cambridge, 1953, p. 186

2 *Criteria of Three-dimensionality of Space*

‘Vom Wesen des Raumes bleibt dem Mathematiker nur die eine Wahrheit in Haenden: dass er ein dreidimensionales Kontinuum ist.’

H. Weyl, *Mathematische Analyse des Raum-problems*

Various criteria of three-dimensionality of space have been put forward at different times. They can be divided into three main classes according to their way of approaching the problem: geometrical, analytical, and physical. In what follows we shall consider and analyse them in this sequence, which may somewhat differ from the chronological order of their origination.

A. GEOMETRICAL CRITERIA

(i) *Triad of orthogonal axes.* The fact that only three mutually perpendicular axes can be constructed at any point of physical space was probably the oldest criterion of three-dimensional space structure. However, this circumstance does not prove the three-dimensionality of space, because a question can naturally be raised: just why perpendicular lines should be preferred above all others so that they could define the number of dimensions? If we choose some other angle between the axes, e.g. 45° , the number of axes inclined to each other through this angle would be 13. This number can determine the number of dimensions just as little as number 3 in the triad of orthogonal axes, unless some decisive arguments can be advanced as to exceptional and superior status of right angles over all other values of angles, apart from convenience or tradition.

In nature, right angles have no preferential status; side-by-side with the cubic crystallographic system, exist also hexagonal, trigonal, tetragonal, rhombic, monoclinic and triclinic systems with very different angles between crystal surfaces and axes. Therefore, the concept of spatial three-dimensionality cannot be based upon the triad of orthogonal axes, unless the preferential status of right angles is assumed by convention, by virtue of their convenience.

(ii) *Criterion of the number of parameters.* At first, this criterion was formulated so that the number of parameters necessary and sufficient to locate any point in space determined the number of dimensions. So, Exner¹ wrote concerning this definition: ‘immer aber sind drei

¹ F. Exner, *Vorlesungen ueber die physikalischen Grundlagen der Naturwissenschaften*, Vienna, 1919, p. 11. The author is indebted to a referee for pointing out this book

voneinander unabhaengige Bestimmungsstuecke erforderlich und das ist das charakteristische Moment, weshalb wir von einem dreidimensionalen Raum sprechen.' Later on, when Peano¹ showed the possibility of accomplishing this with a lesser number of parameters, Brouwer² re-formulated the criterion so that dimensional number should be defined as the number of parameters, by which a manifold in the vicinity of any of its points can be uniquely and continuously represented. He based this definition on the invariance of dimension number.

Both of these definitions rest upon an implicit assumption of the continuous structure of space. At first sight, the relation between continuity and dimensionality might not be obvious. Yet, it will be shown below that a close connection exists between them, and the problem of dimensionality can be solved only after adopting some particular structure of space with respect to its continuity.

(iii) *Recursive definitions of dimensionality.* Poincaré³ determined dimensionality of an n -continuum by means of dividing it by sections of $(n - 1)$ dimensions, and considered the latter as 'walls' limiting the extension of an n -dimensional body or a part of it. By recursion he came to the point, which was assigned zero-dimensionality, without however giving a rigorous definition to the very conception of 'dimension'. The similar recursive definition by Menger⁴ involves only a slight change in formulation and substituted 'boundaries' (Begrenzungen) in place of Poincaré's walls in arbitrarily small vicinities (Umgebungen) of any point.

Both of these recursive definitions deal explicitly with continuum, as was stressed by Poincaré, and assigning three dimensions to space on the basis of these definitions means adoption of the continuous structure of space from the outset.

B. ANALYTICAL CRITERION

An attempt to employ an analytical method only, rather than purely geometrical ones, in considering the problem of space

which contains much important material on the problems of space, time and dimensions, in particular in lectures 3, 9 and 15. Another relevant publication, also kindly pointed out by a referee is the book by P. Frank, *Das Kausalgesetz und seine Grenzen*, Vienna, 1932, p. 23

¹ G. Peano, *Math. Ann.*, 1890, **36**, 157

² L. Brouwer, *Journ. f. reine und angew. Math.*, 1912, **142**, 146

³ H. Poincaré, *Dernières Pensées*, Paris, 1913, p. 67

⁴ K. Menger, *Dimensionstheorie*, Leipzig, 1928, p. 79

dimensionality was undertaken by Eddington.¹ He introduced a so-called *E*-frame in description of physical phenomena and deduced the three-dimensionality of space from its properties consisting in the fact that no more than three perpendicular directions connected by circular rotations are possible in the world possessing pentadic group structure. Hence he concluded: 'Space defined as a domain in which the relativity rotations are circular, is restricted to three dimensions'.

However, the properties of the same pentad indicate the existence of two more dimensions connected by hyperbolic rotations with the other three, which apparently point out at two-dimensionality of time. Eddington himself rejected this conclusion, calling the fifth co-ordinate a phase, a time-analogue, but not the second dimension of time, which is, in his words, 'entirely beyond experience'.

Thus, apart from conventionality of adopting this *E*-frame, acceptance of three-dimensionality of space and rejection of two-dimensionality of time, following from the same frame, cannot be recognised as self-consistent.

C. PHYSICAL CRITERIA

(i) *The form of the known physical laws.* Whitrow considers the form of the inverse-square law of gravitation as a decisive criterion of three-dimensionality of space, for its expression in an n -dimensional space looks otherwise, namely as follows:

$$F = \kappa \frac{Mm}{r^{n-1}} \quad (1)$$

The universal validity of the inverse-square law for gravitational forces is, however, very questionable in the realm of cosmic distances. The spiral structure of galaxies casts serious doubts on it. Although numerous attempts have been undertaken to account for this structure within the framework of the conventional gravitational law, some additional, more or less arbitrary assumptions were postulated which stretched explanations and made them artificial. On the other hand, theories involving plausible modifications of the Newtonian law, as e.g. Vogt's theory,² explain the spiral structure quite naturally. Thus the inverse-square law of gravitation is limited, from the cosmical scale

¹ A. Eddington, *Fundamental Theory*, Cambridge, 1953, p. 124

² H. Vogt, *Die Spiralnebel*, Heidelberg, 1946, p. 156

viewpoint, to insignificant distances. These are, so-to-speak, spatial limits. Besides these, temporal limits may exist, requiring some changes with the passage of time. Such a varying law of gravitation appears in the theories of Milne¹ and Jordan.² The latter adduced much evidence in favour of it.

Thus, being limited spatially and temporally, the inverse-square law of gravitation paraphrasing Whitrow³ himself: 'can be usefully applied to a wide, but not universal, range of natural phenomena'; it cannot be considered as universally valid and proving the three-dimensionality of space.

Moreover, gravitational forces are not the only forces in nature. Turning to nuclear forces we do not find there the inverse-square law in operation. As these are the most powerful forces known, it is not clear just why the criterion of dimensionality should not be based on the form of the nuclear force potential, if an approach from the standpoint of physical forces is to be employed. Then, one would arrive, on the basis of Formula 1, at a quite unexpected number of dimensions, by far in excess of three.

Therefore, the argumentation based on the form of the physical laws can even be turned against the three-dimensionality of space, and in any case cannot be used as positive evidence for this point.

(ii) *The form of a law combining gravitational and electromagnetic fields.* In 1918, Weyl⁴ developed a theory which was intended to bring about unification of the metrical gravitational field, characterised by the fundamental tensor g_{ik} , with the electromagnetic field characterised by the vector ψ_i . Analysing his results, Weyl⁵ came to a conclusion that the unification leading to the simple and harmonic form of electromagnetic laws established by Maxwell was possible only in a four-dimensional world. Reserving one dimension for time, Weyl came finally to the postulation of three-dimensionality of space as an unquestionable truth, as cited above.

The subsequent development of science has not confirmed Weyl's unified theory, and his argumentation for three-dimensionality of space, based upon it, has lost its power.

¹ E. Milne, *Mon. Not.*, 1944, 104, 120

² P. Jordan, *Schwerkraft und Weltall*, Brunswick, 1952, p. 191

³ G. Whitrow, this *Journal*, 1951, 2, p. 58

⁴ H. Weyl, *Sitzber. Preuss. Akad. Wiss.*, 1918, 1, 465

⁵ H. Weyl, *Philosophie d. Mathematik und Wissenschaft*, Munich, 1927, p. 98

3 *Dependence of Space Dimensionality on Continuity*

Λόγον ἔχειν πρὸς ἀλλήλα μεγέθη λέγεται ἂ δύναται
πολλαπλασιαζόμενα ἀλλήλων ὑπερέχειν

Euclid, Fifth Book

Some of the criteria mentioned above depend explicitly or implicitly on the assumption of the continuous space structure and validity of the Archimedean axiom as formulated, for instance, by Euclid.

(i) *Continuity of Space as an Axiom*

‘Qu’est-ce qu’un point de l’espace ? Tout le monde croit le savoir, mais c’est une illusion.’

Poincaré, *La Science et l’Hypothèse*

Does the continuity of space present an empirical fact or an *a priori* property, the truth of which is self-evident ? Neither. There is no empirical evidence whatsoever to this effect, but some facts, to be considered below, point out to the contrary.

That the notion of continuity is not above suspicion, shows the whole history of philosophy from the time of Plato up to the present: controversies between Aristotelian adherents of continuity and their adversaries have never subsided. And in modern philosophy Whitehead¹ expressed this quite unequivocally: ‘The continuity of space apparently rests upon sheer assumption unsupported by any *a priori* or experimental grounds’.

Mathematicians, even those who contributed most to the development of abstract ideas of geometrical continuity as represented by arithmetical continuity of real numbers, also recognised the possibility of different space structure, as was admitted by Dedekind: ² ‘Hat ueberhaupt der Raum eine reale Existenz, so braucht er doch nicht nothwendig stetig zu sein.’ So the status of the continuity notion as a postulate ought to be constantly borne in mind.

With this is closely connected the Archimedean axiom, usually formulated that for any 2 quantities ‘*a*’ and ‘*b*’ there always exists an ‘*n*’ such that $na > b$. If this is re-written as follows: $\frac{b}{n} < a$, the alternative equivalent formulation, as applied to geometrical entities, would sound as follows: for any two fragments of a straight line, ‘*b*’

¹ A. Whitehead, *Essays in Science and Philosophy*, New York, 1947, p. 281

² R. Dedekind, *Stetigkeit und irrationale Zahlen*, Brunswick, 1892, p. 12. In this and other quotations from the old German publications, their original obsolete orthography is preserved

and 'a', however small 'a' may be, there always exists an 'n' such that 'b' being divided by it will be shorter than 'a', approaching zero, i.e. a mathematical point, as $n \rightarrow \infty$. The acceptance of validity of the Archimedean axiom in this form is equivalent to the recognition of mathematical points as meaningful entities in physical space.

But what is a point, asked Poincaré, and quite justly answered that an alleged knowledge of it is nothing but illusion. If definition of the mathematical point as an abstraction does not involve any difficulties, to define a point in our physical space is an extremely hard problem. In its essence, the point in space is an inverse of infinite space, just as zero is an inverse of mathematical infinity. This correlation between two infinities, in the large and small, was very pointedly emphasised by Weyl: ¹ 'Der Raum ist nicht nur in dem Sinne unendlich, dass man in ihm nirgendwo an ein Ende kommt; sondern an jeder Stelle ist er nach innen hinein unendlich, ein Punkt laesst sich nur durch einen ins Unendliche fortschreitenden Teilungsprozess fixieren.' Such an infinite dividing process, although thinkable abstractly, could hardly be applicable to physical space. At any rate, its applicability, i.e. validity of the Archimedean axiom in nature, is in just the same need of experimental confirmation as any other postulate of geometry, as was admitted by Hilbert.²

We postpone to Section 4 (iii), the discussion of physical data pertaining to the structure of space, because they should be considered together with some time properties, which are the subject of Section 4 (i), (ii), and explore now the alternative discontinuous structure of space, in so far as this has a bearing on the problem of its dimensionality.

(ii) Discrete Structure of Space

Unter 'Punkt', 'Gerade' usw. sind in der axiomatischen Geometrie nur inhaltsleere Begriffsschemata zu verstehen. Was ihnen Inhalt gibt, gehoert nicht zur Mathematik.

A. Einstein, *Geometrie und Erfahrung*

Adoption of discrete structure means that we drop the postulate of continuity, as defined by Hilbert³ or Dedekind, and the postulate of Archimedes. We mention both of them, for they are independent of each other, and some non-Archimedean geometries were actually

¹ H. Weyl, *Philosophie d. Mathematik und Wissenschaft*, Munich, 1927, p. 33

² D. Hilbert, *Math. Annal.*, 1918, 78, 405

³ D. Hilbert, *Grundlagen der Geometrie*, Leipzig, 1913, 4th edn., p. 21

devised in which continuity was preserved, as those of Veronese, Hilbert, etc.¹

To renounce these postulates means the following: the location of any physical point in space can be determined only with a finite accuracy down to a certain minimum value l_0 ; distances of a lesser value have no physical sense. The physical space cannot be subdivided beyond a certain limit. These ideas were extensively developed by March.²

We shall now apply this conception to our problem of space dimensionality. Let us consider first the location of a point in a plane. For this purpose, let us draw on this plane an Archimedean spiral

$$\rho = l_0 \theta \quad (1)$$

with separation of two neighbouring windings being l_0 (Fig. 1). Any

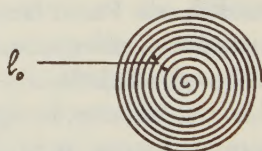


FIG. 1.

point on the plane can be located by the length of the spiral arc from the origin to this point, expressed by the formula:

$$L = \frac{l_0}{2} \left[\theta \sqrt{1 + \theta^2} + \text{Ar sinh } \theta \right] \quad (2)$$

There is no sense of speaking about the points situated between any two windings of the spiral, since they are indistinguishable from the points lying on the windings themselves, because their separation is limited by l_0 . Therefore, this spiral covers the entire area of the plane, as if the spiral had windings which possess a very small width l_0 and adhere closely to each other so that no spacing is left between them. The location of any physical point on the plane is determined uniquely and continuously (in a sense that no abrupt transitions occur) by a single parameter L .

Now extend this conception to space.

Let us draw this spiral on a plane and then roll the plane into a similar spiral surface with the same spacing of windings equal to l_0

¹ F. Enriques, *Enz. Math. Wiss.*, Leipzig, 1907, 3, 1, 1, p. 117

² A. March, *Zeit. f. Phys.*, 1937, 104, 93, 161; 105, 620; 106, 49, 291

(Fig. 2). If the curve were of infinite length, the resulting spatial spiral will fill the whole of space for the same reason as above: the points between the windings of this surface are indistinguishable from the points in the surface itself. Thus in this case, too, the location of any physical point in space is uniquely and continuously determined by a single parameter, the length of the curve from the origin.

A possible objection can be raised against this procedure, namely, that our plane must be rolled, as otherwise no extension of it could produce space. It is true, but rolling does not affect dimensionality. A cylindrical surface obtained from a plane does not differ from the latter either metrically or dimensionally. In our case also there is no difference in dimensional respect between the flat and rolled surfaces.

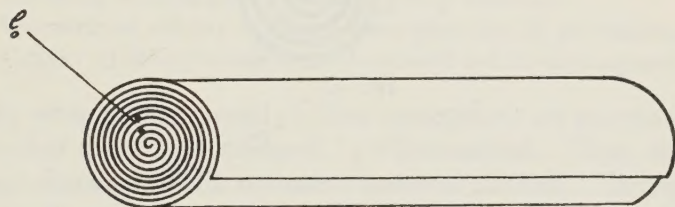


FIG. 2.

(iii) *How many Dimensions has Discrete Space ?*

‘ ein diskontinuierlicher kompakter Raum ist nulldimensional ’.

K. Menger, *Dimensionstheorie*

If we wished now to apply the criteria listed above to this discrete space model, we would suffer failure. It is easy to see that neither Brouwer's criterion of parameter number nor Poincaré's and Menger's recursive definitions are applicable to discrete space.

Indeed, from this possibility of representing it by one parameter, are we entitled to conclude that space is one-dimensional ? Not at all. This would be the case, if the spiral itself were a continuous line. However, by adopting discontinuous structure of space, we should consider the spiral line as also being discrete, which is composed of physical points separated by the same least length l_0 . In this case, both the spiral line and the space generated by it may be expressed in terms of the number of physical points composing them. Such a system, consisting of discrete points, will not be a continuum any longer; it will be called a ‘ zero-dimensional cell system ’ and denoted by Z^0 .

As such, this system will be the subject of combinatory rather than continuous Analysis situs.

Newman,¹ in his development of combinatory Analysis situs, used the theorem of superposition as an indicator for the topological equivalence of complexes. If we apply the criterion of superposition to our spirals, we shall note the following: a spiral with widely separated windings, pictured in Fig. 3 (conventionally called one-dimensional), can be superposed upon the closely-wound spiral covering the entire area of a plane as described above, shown in Fig. 1; a plane rolled with spacings, pictured in Fig. 4 (conventionally called



FIG. 3.

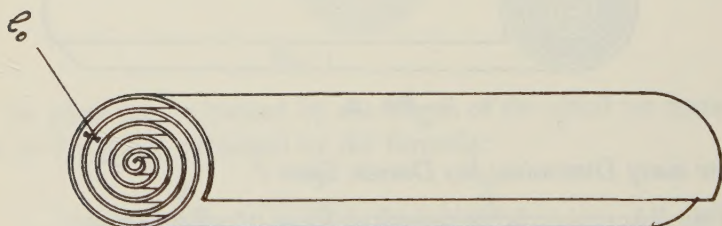


FIG. 4.

two-dimensional), can be superposed upon the closely-packed surface filling the whole space, shown in Fig. 2. Therefore, all these sets are topologically equivalent, although they would have different numbers of dimensions, had they been continuous manifolds.

Another criterion, 'Verwandschaft', was proposed by Tietze:² two Z^0 are 'verwandt', if they possess an equal number of points. Applying this criterion to our system described above, we can always compare spirals of equal length and consequently containing the same number of points. This property, Verwandschaft, is sufficient for homeomorphy of zero-dimensional complexes, which indicates their topological equivalence.

¹ M. Newman, *Math. Ann.*, 1928, **99**, 399

² H. Tietze, L. Vietoris, *Enz. Math. Wiss.*, Leipzig, 1930, **3**, 1, 2, p. 207

PHYSICAL SPACE AND TIME

The concept of dimensionality, in the conventional meaning, loses all sense, for it is not applicable to discrete spaces. Thus, we have shown that the problem of dimensionality of our physical space cannot and should not be solved before and independently from the problem of continuity.

We have analysed several criteria proposed for the establishment of a certain number; i.e. number '3', as the number of space dimensions, and we have seen that all rest upon some explicit or implicit assumptions or conventions, in addition to Eddington's 'ordering relation' or interval. These criteria were the following:

1. Preferential status of orthogonal axes to all others,
2. Continuous structure of space and significance of geometrical points as physical points (Archimedean axiom),
3. Adoption of Eddington's pentadic group structure,
4. Unrestricted validity of the inverse-square law of gravitation,
5. Validity of Weyl's unification of metrical and electromagnetic fields.

Only when one or several of these assumptions are postulated, can the physical space be considered '3'-dimensional. Thus, the conventional character of this statement becomes obvious. The axiom of the three-dimensional nature of space is neither an *a priori* truth, nor an experimental fact, and so it does not differ in this respect from all other geometrical axioms, which by Poincaré's definition:¹ 'ne sont donc ni des jugements synthétiques à priori ni des faits expérimentaux. Ce sont des conventions.'

The number '3' of spatial dimensions is not an absolute number, nor is it intrinsic in the very nature of space. If it were so, this number would inevitably manifest itself in some dimensionless constant of nature, as this was already recognised by Ehrenfest.² No such connection between the number '3' and any of the universal dimensionless constants has ever been proved.

Therefore, the final conclusion of our investigation can be formulated as follows: the three-dimensionality of space is not its absolute characteristic, but a property conventionally ascribed to space in view of assuming tacitly at least one of the above-mentioned axioms, in particular the axiom of continuity. Thus the treatment of the problem, as was pointed out by a referee, basically pivots on the idea that the number of dimensions ascribed to space depends upon some theory.

¹ H. Poincaré, *La Science et l'Hypothèse*, Paris, 1920, p. 106

² P. Ehrenfest, *Annal. d. Phys.*, 4 Folge, 1920, 61, 440

4 *Some Problems in Topology of Time*

'What we, as scientists, know about time itself is very little indeed. . . . Time moves, or we move through it, in one direction only.'

G. Clemence, *Time and Its Measurement*

The sacred awe which implacable time inspires in us is the main reason preventing our learning more of its nature and properties. Perhaps the Clemence view that time study constitutes the subject of philosophy rather than science but contributes to maintaining this aureola of transcendentalism enveloping time. This viewpoint seems to have become obsolete since the ushering in of relativity, which dethroned *χρόνος* and deprived him of his mystic halo of being absolute.

(i) *Space-Time Relationship*

Gurnemanz: 'Du siehst, mein Sohn, zum Raum wird hier die Zeit.'

R. Wagner, *Parsifal*, 1st act

Any discussion of space properties would be essentially incomplete, if space-time relationship were not taken into account. Both of them together constitute an inseparable framework of our physical Universe, and space in itself 'sinks to a mere shadow', using the famous Minkowski expression.¹

Therefore, we expand the scope of this essay to consider some problems of space-time relationship and dimensionality of time, but not considering for the present the problems of time nature and its unidirectional sense of flow.

First of all, one fundamental question has to be answered: whether the union of space and time has any physical meaning, or does it merely represent a convenient mathematical tool possessing no more physical significance than time-temperature or time-pressure combinations in weather diagrams. The latter view is often maintained, and it is sometimes stated that 'space and time have almost nothing in common, beyond their interchangeability in certain equations'.² If this viewpoint were based on a firm foundation, the space-time union would be deprived of any physical meaning, the properties of one of them would have no connection whatsoever with the properties of

¹ H. Minkowski, *Phys. Zeit.*, 1909, **10**, 104

² G. Clemence, *Amer. Scientist*, 1952, **40**, 260

the other, and changes affecting one of them could occur without necessarily simultaneously affecting the other.

There exist many facts and phenomena which indicate the opposite. A close physical relationship between them is manifested first of all in the existence of the universal constant 'C' having dimension of ST^{-1} , which was stressed by Heisenberg,¹ although its significance is not as yet fully understood. The entire edifice of special relativity rests upon this physical relationship of space and time. One particular feature is worthy of emphasis at this point, for it will be important in the further discussion of this problem.

The world line and time-axis of a system moving at velocity v with respect to another system of reference will constitute an angle to the time-axis of the latter:

$$\psi = i Ar \tanh \frac{v}{c} \quad (3)$$

This turnability of time-axis is of great fundamental importance. It means that any system has theoretically not only a single line in time direction, which must be unavoidably pursued, but a whole infinity of them restricted only by 'isotropic' lines, with which they constitute an angle equal to ∞ .²

In this respect, the time property is similar to that of space, in which infinity of motion directions is possible. It can be argued that this similarity is only superficial, and the motion through time is not as easy as the motion through space, for 'We can manipulate spatial separation in a way impossible with time, which cannot be juggled with',³ for time is irreversible and it is impossible to come back to the same point in time, whereas one can readily return to the same point in space. Actually, however, this distinction is only apparent. The identification of one spatial point at two different time instants is just as little possible, as the identification of one temporal point in two different points of space. As the simultaneity in two widely separated points cannot be defined because of the non-existence of absolute time, so the identity of space points in two time instants cannot be determined because of the non-existence of absolute space. Thus, in this respect, the properties of space and time are also similar.

¹ W. Heisenberg, *Das Plancksche Wirkungsquantum*, Berlin, 1945, p. 13

² F. Klein, *Vorlesungen ueber nicht-euklidische Geometrie*, Berlin, 1928, p. 143

³ M. Johnson, *Amer. Scientist*, 1951, 39, 412

Both space and time are liable to distortion by a gravitational field, as can be seen by inspecting Schwarzschild's solution¹ of general relativity equations, and by the presence of electric charges, as shown in Jordan's solution.²

Thus any factor, be it the state of motion, acceleration, gravitational field or electric charges, etc., which affects space, also affects time, and in this respect they are also alike.

All this strengthens the view, that first; the union of space and time is not merely their mathematical amalgamation in some formulæ, but a physically significant close interrelation, and second; that their properties are so strikingly similar in many respects that one dictum by Jeans³ seems to be fully justified: 'Space and time themselves must at least be of the same general nature'.

(ii) Dimensionality of Time

'There is no difference between Time and Space except that our consciousness moves along it'.

H. Wells, *Time Machine*

There is but one number of time dimension. This is a general opinion which is adopted as readily as the number '3' for space dimensions. Yet, in distinction to numerous investigations dealing with the space dimensional theory, there is no general dimensional time theory. Its one-dimensionality is either assumed as a self-evident fact or is based on one of the following considerations.

Reichenbach⁴ connects time flow with causation chains in the world, Eddington⁵ and Schroedinger⁶ with entropy increase, although with a difference that the former considers it as universal and unidirectional, whereas the latter considers it as local and denies the existence of Eddington's 'arrow of time'. On the main point, they agree that time has just one dimension.

Although this conception is the most widely represented, it is not the only one possible. Several authors have written concerning two-

¹ K. Schwarzschild, *Sitzber. Preuss. Akad. Wiss.*, 1916, I, 189

² P. Jordan, *Schwerkraft und Weltall*, Brunswick, 1952, p. 85

³ J. Jeans, *Physics and Philosophy*, Cambridge, 1948, p. 68

⁴ H. Reichenbach, *The Philosophical Significance of the Theory of Relativity*; Albert Einstein: *Philosopher-Scientist*, New York, 1951, p. 289

⁵ A. Eddington, *The Philosophy of Physical Science*, Cambridge, 1949, p. 198

⁶ E. Schroedinger, *Nature*, 1944, 153, 704

and three-dimensional time.¹ The latest treatment of time as a two-dimensional entity was published by Dobbs² in the pages of this *Journal*. Dobbs introduced a distinction between the 'transition time' and 'phase time' and treated the main points of his theory, in particular, the 'specious present' problem from the psychological viewpoint.

Adherents of the conventional one-dimensional theory of time frequently employ psychological and physiological criteria, as did Eddington³ in connecting our perception of the flight of time with molecular processes in the brain.

This approach should not become a starting point in any physical theory of time, however suitable it may prove to be in psychology. The time of physics should be clearly and unequivocally distinguished from the perceptual time of psychology. Our primary concern here is the former.

What must be understood under the name of 'physical time'? In our opinion, it is a factual constituent of the world substratum, be it called 'aether' or otherwise, which is on the same footing as another constituent, space.

What is the dimensionality of this physical time?

Jeans⁴ holds it as one-dimensional, as physical events can be arranged in a linear sequence. There are, however, several fundamental phenomena not accounted for within the framework of present conceptions: existence of opposite electric charges, positive and negative; opposite interactions, attraction and repulsion, etc. Eddington's 'Fundamental Theory', which contains many deep thoughts, as justly pointed out by Whitrow, introduced the conception of opposite 'chiralities' having a direct bearing on electric charges. Yet, it leads to the notion of a two-dimensional physical time, although the author did not want to admit this.

This is true, of course, if time is considered as a continuous quantity, as in the conventional theory. So, time is characterised by Weyl⁵ as the most fundamental continuum, in which the Archimedean axiom

¹ A. Calinon, *Etude de cinématique à 2 et à 3 dimensions*, Paris, 1890; M. Boucher, *Essai sur l'Hyperspace, le temps, la matière et l'énergie*, Paris, 1927, 3rd edn., p. 62; G. Bragdon, *Fourdimensional vistas*, New York, 1930, 3rd edn., p. 76

² H. Dobbs, this *Journal*, 1952, 2, 122 and 177

³ A. Eddington, *Space, Time and Gravitation*, Cambridge, 1953, p. 13

⁴ J. Jeans, *Physics and Philosophy*, Cambridge, 1948, p. 57

⁵ H. Weyl, *Das Kontinuum*, Leipzig, 1918, p. 67

is unconditionally valid, and the 'Zeitpunkt' represents a basic conceptional category.

This time structure, however, was assumed on the postulational basis. As soon as we recognise that this is an axiom, which also has no *a priori* or experimental grounds, we can explore the possibility of discarding it and adopting its opposite, discrete structure. In this case, too, the usual meaning of dimensionality will be lost, and physical discontinuous time will be represented by a zero-dimensional complex of cells, which are physical time instants having some minimum extension or duration τ_0 .

If both space and time are zero-dimensional cell complexes, does this mean that they are dimensionally equivalent? So-called 'common sense' discards such a conclusion for a simple reason: there are many directions in space and only one in time. Our 'common sense' judges on the basis of our perceptions, and the perception of different dimensions rests upon our ability to differentiate between various directions. This ability is rooted in physiological and psychological properties of the human being. So, he is equipped with semicircular canals to discriminate between spatial directions. The rôle of these canals was very extensively investigated and summarised by Cyon¹ as follows: 'Die durch die Erregung der Bogengänge erzeugten Empfindungen sind Richtung—und Raumempfindungen. Diese Empfindungen dienen dem Menschen zur Bildung der Vorstellung von einem dreidimensionalen Raume, auf den er seinen Seh—und Tastraum projicirt'. A hypothetical human being, deprived of these internal organs and whose body was of a perfectly symmetric (e.g. globular) shape, could hardly reach the conception of a three-dimensional space.

Turning now to the perception of time, we can see that the human being is very poorly equipped in his time perception in comparison with that of space. Although many directions in time are theoretically possible, due to turnability of the time-axis with high-speed motion, there is no organ in the human body to discriminate between them. Even one direction in time, that is actually pursued, is not directly perceived, because one single point, the present instant, is not sufficient; it does not define a direction, for which at least two points are needed. Our immediate awareness in time is limited to the present instant only (remembrance of past instants and anticipation of future ones are not comparable with direct perception of spatial points

¹ E. v. Cyon, *Ohrlabyrinth, Raumsinn und Orientierung*, Bonn, 1900, p. 275

already passed and forthcoming, which can be immediately observed). There is no time perception organ in the human body. Some hypothetical being, provided with such an organ and possessing an ability to be directly aware of past or future instants, would devise quite a different notion of time, perhaps two- or multi-dimensional.

Thus, we ascribe different numbers of dimensions to space and time not because of an intrinsic difference between their dimensions, but because of specific peculiarities in our psychological perception of time, i.e. because 'our consciousness moves along it'.

(iii) *Discontinuity of Space and Time*

'We still use it (concept of the continuum) for space and time; . . . but it may very well turn out to be out of place for physical space and physical time'.

E. Schroedinger, *Nature and the Greeks*

We discarded the postulate of continuity and Archimedean postulate and adopted tentatively the opposite; the hypothesis of discrete structure of space and time; that is, the existence of the least length l_0 and shortest duration τ_0 . Does this hypothesis have any grounds? First of all, we have to mention that this notion is not new at all. The first consequent proponents of discontinuous structure of both space and time, although from a purely philosophico-theological viewpoint, were medieval Arabian interpreters of the Koran, Mutakallimun.¹ A hundred years ago the discrete structure of space was surmised by Riemann² as one of the possible grounds for its metric properties.

In recent times, strong arguments in its favour were provided by some inconsistencies in the modern physical theory. The most significant of them is infinite self-energy of the point-electron, which indicates the meaningless application of zero distances to the real physical world. Although many ingenious attempts have been made to avoid this and other similar infinities and divergences, this struggle against infinities very much resembles the famous Don Quixotian struggle against windmills, for they were first imagined as real enemies and then attacked with a zeal deserving more constructive application.

As early as in 1930, Ambarzumian³ proposed to eliminate the difficulty of electron infinite self-energy in a reasonable way, by

¹ K. Lasswitz, *Geschichte der Atomistik*, Hamburg, 1890, I, p. 134

² B. Riemann's *gesammelte mathematische Werke*, ed. by Weber, Leipzig, 1876, p. 254

³ V. Ambarzumian, D. Iwanenko, *Zeit. f. Phys.*, 1930, **64**, 563

introducing a spatial lattice structure. Schild¹ extended this idea to spatio-temporal lattice with a fundamental length of the order of 10^{-13} cm. and proved that the model of discrete space-time 'admits a surprisingly large number of Lorentz transformations'. March, Flint² and others also developed theories of discrete space-time, which proved to be self-consistent and free of any logical or physical contradictions. This circumstance alone indicates that the postulate of space-time continuity cannot be proven by 'reductio ad absurdum', by adopting its opposite; i.e. discontinuous structure.

If discrete structure of space is adopted as a working hypothesis, time must be assumed as discontinuous, too, in view of similarities pointed out above and their interrelation. Indeed, with space and time being inseparable and if space is discontinuous, time cannot be continuous, for otherwise there is no way of ordering continuous time instants to discrete space points; in this case, infinity of instants in the time continuum would be left without their spatial counterparts.

Another argument was cited in the above mentioned Ambarzumian article: the existence of the least length l_0 implies the existence of the least wavelength, and this leads to the maximum possible frequency or the least time interval $\tau_0 = \frac{l_0}{C}$. Thus it is a necessary logical consequence, that discreteness of time must accompany discreteness of space.

Are there any indications in Nature itself that these least quantities of space and time, their so-to-speak quanta, have actual physical reality? It is perhaps not without interest to cite a few figures indicating the possibility that this is the case.

Let us consider a physical point-instant, or quantum of space-time, as an irreducible spatio-temporal volume defined by the expression:

$$V_{iv} = \frac{4}{3}\pi l_0^3 \tau_0$$

Assuming l_0 to be of the order of magnitude of a proton radius, i.e. $\sim 1.5 \times 10^{-13}$ cm., and the value of $\tau_0 = \frac{l_0}{C} = 0.5 \times 10^{-23}$ sec., we obtain the value of $V_{iv} = 7.1 \times 10^{-62}$ cm.³ sec. If we imagine this spatio-temporal quantum to be filled with matter of nuclear density, $\sigma \cong 10^{14}$ g/cm.³, its energy content will be

$$V_{iv} \sigma C^2 \cong 6.4 \times 10^{-27} \text{ erg-sec.},$$

¹ A. Schild, *Canad. Journ. of Math.*, 1949, **1**, 29

² H. Flint, *Phys. Rev.*, 1948, **74**, 209

which is of the same order of magnitude as Planck's quantum $h = 6.624 \times 10^{-27}$ erg-sec. An idea suggests itself that the elementary quantum of action has something to do with discrete structure of the world, and with the elementary and irreducible quantum of space-time; that is, with the natural standard of length in the pure form, as was suggested by Whyte.¹

Let us now turn to the outer limits of space and time. Weyl² once put forward an idea that the non-Euclidicity of world space may mean that it has not only outer, but also inner limits. We shall now reverse his argument with respect to time. The limitation of time in the small to some indivisible quanta τ_0 may mean the existence of outer limits; that is, the existence of the longest time interval possible, instead of infinite time, which would be just another logical consequence of renouncing the Archimedean axiom.

We have thus come, in another way, to the same conception of finite time, which this author suggested in this *Journal*³ some time ago.

Now let us determine, in what relation the world of finite extension in space and time stands to its discrete structure. Assuming the value of space radius R to be equal to 3.4×10^9 light years, and the radius of time curvature $\rho = 3.4 \times 10^9$ years, as determined by this author⁴ the spatial volume of the closed Riemannian world will be

$$V = 2\pi^2 R^3 = 8 \times 10^{83} \text{ cm.}^3,$$

the duration of the finite circular time will be

$$T = 2\pi\rho = 6 \times 10^{17} \text{ sec.},$$

and the spatio-temporal volume will amount to $\sim 10^{101} \text{ cm.}^3 \text{ sec.}$ With the value of one spatio-temporal quantum of the order of $10^{-61} \text{ cm.}^3 \text{ sec.}$, as estimated above, the number of these units in the world will be $\sim 10^{162}$.

The so-called 'cosmic number' N , being the number of elementary particles in the cosmos, was estimated by Eddington⁵ to be of the order of 10^{60} . There is perhaps some relation between these two numbers, the latter being possibly a square root of the former, thereby connecting the atomistic structure of matter with discrete, quantised structure of space-time. Weyl⁶ once wrote in another connection:

¹ L. L. Whyte, this *Journal*, 1951, 1, 303

² H. Weyl, *Raum. Zeit. Material*, Berlin, 1921, 4th edn., p. 238

³ 1954, 5, 237

⁴ Ibid., p. 245

⁵ A. Eddington, *The Philosophy of Physical Science*, Cambridge, 1949, p. 177

⁶ H. Weyl, *Naturwiss.*, 1934, 22, 145

‘Das Auftreten der Wurzel ist nicht unverstaendlich, . . . In den Statistischen Gesetzen tritt ja haeufig die Quadratwurzel aus der grossen Zahl der Einzelfaelle auf.’ This consideration is applicable also to the present case.

Of course, only orders of magnitude can be compared; the exact coincidence of values in both cases above are not to be expected, in view of our still imperfect knowledge of the numerical values of lengths involved, and even more, our ignorance of the geometric structure of the world as a whole and its elementary quanta of space-time.

5 Conclusions

‘solche Untersuchungen, welche, wie die hier gefuehrte, von allgemeinen Begriffen ausgehen, koennen nur dazu dienen, dass diese Arbeit nicht durch die Beschraenktheit der Begriffe gehindert und der Fortschritt im Erkennen des Zusammenhangs der Dinge nicht durch ueberlieferte Vorurtheile gehemmt wird.’

B. Riemann, *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen*

The problem of dimensionality of our physical space and time is closely connected with and depends on the solution of the other fundamental problem: continuity or discontinuity of their structure. Both of them are topological problems, which deserve more attention than was paid them thus far.

The numbers ‘3’ and ‘1’ are conventionally ascribed to dimensions of space and time respectively, when continuous structure of both was presumed as an axiom. The choice of these particular numbers was determined by the physiological and psychological properties of the human body (asymmetry, existence of space organs, non-existence of time organs, etc.). As soon as the postulate of continuity is dropped and physical spatio-temporal point-instants are assumed to possess some finite extension, dimensionality of both space and time loses its conventional meaning and absolute character.

If the Archimedean axiom is not applicable to our physical world, and its spatial and temporal dimensions are finite, the world’s content is characterised not by its volume or any other dimensional quantity, but by the number of its quanta or spatio-temporal physical points; i.e. by a dimensionless quantity or a pure number, in accordance with the view advocated as early as twenty-five centuries ago by the Pythagorean school.

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Hereby it is not denied, that for the majority of today's problems, it is convenient to ascribe three dimensions to space, as admitted by Poincaré, and one dimension to time. However, the categorical statement that 'our physical world has three spatial and one temporal dimensions' should be replaced by a more modest one: 'At the present stage of human knowledge, most physical phenomena can be described as if happening in a three-dimensional space and one-dimensional time, assuming conventionally they both are continuous, or in a world, the line-element of which is expressed by a differential equation of signature $(+++ -)$ '.

Let us draw an analogy with Plato's prisoners chained in a cave and capable of seeing only shadows of the real objects outside of the cave reflected on its back wall. Their viewpoint would probably be characterised as follows: 'our physical world consists of two-dimensional colourless beings moving silently before our eyes'. Let us imagine now that some ingenious inventor in this cave devised a device, which renders shadows stereoscopic, another devised a method of reflecting colours, etc. The new picture of the physical world composed by the cave inhabitants would radically differ from the initial one.

Is it impossible, that analogous stereoscopic or other, figuratively speaking, improvements may happen in our description of physical reality?

Is it absolutely improbable, that a second, conventionally speaking, time dimension in one form or another may appear in the expression of the line-element?

Can an idea that the expression of line-element in differential form may be replaced by a difference equation, in which finite differences reflecting quantised properties of the world substratum will take over the former rôle of differentials, be discarded as unthinkable?

These and perhaps other developments cannot be denied *a priori*. Therefore, the statement regarding ' $3 + 1$ ' dimensionality of our physical world must not be considered as an unquestionable and ultimate truth, excluding all alternatives, if 'the progress in understanding the connection of things,—as Riemann declared once—should not be hampered by traditional prejudices'.

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TWO EVOLUTIONARY THEORIES (I) ★

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I *The Two Theories*

1959 will find the majority of Western biologists assenting with renewed enthusiasm to the basic principles enunciated a century ago in *The Origin of Species*. But there continues to be an heretical minority, and, moreover, the heretics speak with authority and vigour. When, in turn, the orthodox pause to answer the dissenters' arguments, they offer an illuminating paradigm of scientific controversy: illuminating both for a study of the nature of scientific disputes in general and for the epistemological problems inherent in evolutionary controversy in particular.

I propose to limit myself here to the analysis of one such controversy: the disagreement between the theories of evolution stated by two palaeontologists, G. G. Simpson in *Major Features of Evolution* (1953) and O. H. Schindewolf in *Grundfragen der Paläontologie* (1950).¹

Professor Simpson is the principal American spokesman of neo-Darwinism, or, as he now prefers to call it, the 'synthetic' theory, and his *Major Features* is certainly one of the most carefully elaborated statements of this position.² The very fact that he speaks as a palaeontologist lends weight to that statement. Darwin himself had trouble with palaeontology;³ and there have been palaeontologists ever since who have failed to adhere to the Darwinian position even

★ Received 13.i.1958

¹ I have drawn also to some extent on other writings of the same authors, notably G. G. Simpson's *Meaning of Evolution* (1949) and two addresses by Schindewolf: 'Evolution vom Standpunkt eines Paläontologen', *Schweiz. Pal. Gesell.*, 1952, 374-386, and 'Evolution im Lichte der Paläontologie', *Comptes Rendus, Congrès Geol. Internat.*, 1954, 93-107.

² It seems to me fair to call Simpson's theory neo-Darwinism if one recognises the differences introduced by the Mendelian basis of the newer type of theory. In essentials it is still Darwinism.

³ *Origin of Species*, Chapters X, and 6th edition, Chapter VII, answer to St George Mivart on abrupt transitions of form.

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when it had become an orthodoxy. Darwinism—and in this neo-Darwinism is no different—presents a picture of life streaming endlessly forward, confined in this or that channel only by the combined effect of environmental change and natural selection, which selects the useful by breeding out the less useful. This picture, however, has seemed to some palaeontologists a distortion of the fossil record, and particularly in two ways.¹ First, the record itself is full of gaps; forms appear and disappear suddenly. There are a few famous transitional forms, like *Archaeopteryx*, but not nearly all there should be if evolution really happened as gradually as the Darwinians think. Secondly, such continuities as the fossil record does exhibit often seem to be unrelated to utility (or in modern language to adaptation) and hence to lie beyond the control of natural selection. The stock examples of this are found in the sabretooth tiger and the Irish elk. Respectively they have teeth or antlers that go on and on getting bigger and bigger even after they become a nuisance, and this was said to illustrate the phenomenon of 'orthogenesis'. This concept is still used by some scientists (including Schindewolf), but the problem it raises really forms part of a larger question: are there important phenomena in evolution which appear to be non-adaptive, and which, therefore, cannot be reasonably interpreted under the aegis of selection? Up to recent years, at least, many palaeontologists thought there were such phenomena: they thought, in other words, that the concepts of adaptation and selection were not adequate for the interpretation of their subject matter.

Against these opinions we have Simpson, himself a most distinguished palaeontologist, the foremost living authority on equid history and a pioneer in work on rates of evolution, lending the weight of his authority to the neo-Darwinian view. He sees evolution as a continuous series of minute changes in innumerable directions, in which all alterations of any significance, larger as well as smaller, quicker as well as slower, are determined by the great co-operating 'pressures' of mutation, geographical isolation, and selection, with adaptation as the universal effect, and criterion, of systematic change. The basic concept, ultimately, is variation in the occurrence of genes; out of such variations all the systematic relations of living things have been gradually evolved.

¹ See for example H. L. Hawkins, *Rep. Brit. Ass.* (Presidential Address to the Geological Section), 1936, pp. 57 *et seq.*; H. F. Osborn, *U.S. Geol. Surv. Mon.*, 1929, 55; *Am. Nat.*, 1934, 68, 193-235; L. F. Spath, *Biol. Rev.*, 1933, 81, 418-462.

There are a number of prominent biologists who are dissatisfied with this view;¹ Simpson, in *Major Features*, takes special pains to reply to the critique of one of them: the German palaeontologist Schindewolf; and it is with their disagreement that I propose to deal.

Schindewolf finds the fossil record full of striking discontinuities, even where there can be no serious question of gaps in the record; new structures just suddenly turn up. Such new structures, moreover—for instance, the limbs of tetrapods—were not, he believes, in themselves adaptive. Land dwellers *might* have started with two legs, or half a dozen, or, octopus-like, with a ring of tentacles. How can we say that four is the best-adapted number, or that the pentadactyl ground-plan is the best-adapted pattern for such limbs in all the great variety of circumstances in which they have come to be used? This happens to be the characteristic tetrapod 'Bauplan', but there is no conceivable reason of utility why this number and no other, this blueprint and no other² should be found. Once present, of course, these four pentadactyl limbs did indeed develop in specialised, adaptive ways: for running, digging, jumping, etc. Their common structure, however, was not in itself, in Schindewolf's view, an adaptation, it was simply a new type of organisation, four-footedness, capable of numerous and varied adaptations.

Furthermore, Schindewolf agrees with the older palaeontologists that within each type, once it has appeared, there is a progressive, orthogenetic development. In fact, there is a rhythm analogous to that of birth, maturation, and senescence: the sudden appearance of a new type, its orthogenetic advance, and finally a stage of the breaking up of types which usually leads to extinction.³ The choice between a paraxonic and mesaxonic limb, for example, appears to be, from the point of view of adaptation, a matter of indifference. For speed of locomotion, either will do. Once the choice was made, however, there was a progressive, orthogenetic trend within each form, to the

¹ I may mention, in this country, four scientists who have expressed themselves as, in various ways, dissatisfied with the new synthesis (although I do not suggest that they would be in agreement, either with what I am saying here, or with one another): Professor H. Graham Cannon of Manchester, Professor Paul G. 'Espinasse of Hull, Professor Ronald Good of Hull, and Professor C. H. Waddington of the Institute of Animal Genetics in Edinburgh.

² The same point is made in a different context in Professor Cannon's recent book, *The Evolution of Living Things*, Manchester, 1958.

³ Schindewolf calls these stages *typogenesis*, *typostasis*, and *typolysis*, and his theory as a whole *typostrophism*.

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horse on the one hand, and the camel on the other. The major type—i.e. the structural plan of artiodactyl as against perissodactyl limb—had to come first, and within this the development of specialised forms occurs—e.g. cows, pigs, and camels as against horses, rhinos, and elephants.

Notice, moreover, that in each case the more general category appears first, and specialisation is specialisation *within* it. Whereas for Simpson it is a question of particular small changes adding up to big ones, for Schindewolf the big changes in structure come first, the new general structures breaking down thereafter into more special adaptations. This is related, as we shall see, to the fact that Schindewolf looks at structures first, putting morphology ahead of phylogeny, while Simpson, like all Darwinians, wants to make phylogeny—the history of life—the one foundation on which all the rest of biology is built.

2 *Parallelism of the Two Theories*

We have here, then, a nicely balanced pair of theories: one stressing continuity and the adaptive character of all evolutionary change, the other stressing discontinuity (novelty of forms) and the non-adaptive character of major changes. The two writers disagree seldom, if at all, about the 'facts', yet each rejects emphatically the other's inferences from the facts and is at pains to refute them. Consider briefly how the two views balance one another in this respect.

(a) Sometimes the very same matter of fact seems to the two of them to provide evidence for diametrically opposite conclusions. For instance, the fact that some very old genera, like *Lingula*, still survive, is proof, for Simpson, that evolution does not exhibit the general pattern of racial life and death: it cannot be said to go on in a cyclical fashion if some old survivors are left happily in their ancient and well-established niches.¹ The same sort of fact is proof, for Schindewolf, that selection is by no means all-powerful, since if it were it would wipe out all simpler forms in favour of the more advanced, i.e. more efficient.²

(b) Each one accuses the other of committing the same or similar fallacies. About the question whether higher categories—orders, classes, phyla—originate before or after species, each one accuses the

¹ *Meaning of Evolution*, p. 192; cf. *Major Features of Evolution*, p. 233

² *Grundfragen der Paläontologie*, p. 404

other, in nearly the same words, of arguing retrospectively, and reading into the record what he himself wants to put there.¹ Did the class of birds originate, in one step, with *Archaeopteryx*, before the innumerable species and sub-species and varieties of birds had developed? Simpson says *Archaeopteryx* was a species like any other, originating by normal speciation from other reptilian species; only when we look back over the whole vista of evolution do we say, this particular species was the first of what has turned out to be a new class.² Schindewolf says, *Archaeopteryx* is a bird: i.e. an animal that flies by means of feathers. There were never any before and there have been a great many since—but the transition, from class Reptilia to class Aves, happened then and there.³ The principle, flight by means of feathers, was genuinely new. Only the neo-Darwinians, late in time, come and conjure away its essential novelty.

Or, again, each accuses the other of unnecessary and mystifying assumptions. Simpson considers that orthogenesis smacks of 'purposiveness'⁴—the worst possible name-calling in evolutionary controversy; and the division of the history of a type into stages analogous to birth, maturity, and old age, he considers viciously anthropomorphic and unjustified by the facts.⁵ Schindewolf, on the other hand, insists that while his use of orthogenesis and of the type concept is purely descriptive,⁶ nothing could be more mystifying than the prophetic vision of selection which the neo-Darwinians implicitly assume: the idea that selection selects what *will* be advantageous in another ten million years, and thus foresees advantages that will accrue to the remote descendants of the forms selected.⁷

True, a neo-Darwinian, accused thus baldly, would simply deny that he makes any such preposterous assumption. Selection, he says, selects what is useful in *each* generation, otherwise it would not be selected. There is no 'foresight' involved, but only the automatic and immediate control of variation through utility. Yet there is nevertheless some plausibility in Schindewolf's criticism; for there are many cases in which Darwinians are forced to relax this basic principle, and to loosen the connection between adaptation and selection. It

¹ Cf. *ibid.*, p. 273, and *Major Features of Evolution*, p. 350

² *Major Features of Evolution*, 342, 370; cf. pp. 347, 350

³ *Grundfragen der Paläontologie*, p. 126; cf. pp. 201, 277

⁴ *Major Features of Evolution*, p. 268

⁵ *Ibid.*, p. 233

⁶ *Grundfragen der Paläontologie*, pp. 430-431

⁷ *Ibid.*, pp. 413, 430-431

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seems highly unlikely, for example, that the very minutest beginnings of ultimately co-adaptive structures, like the appendages of crustaceans, should have been of immediate and constant advantage to their bearers.¹ What does neo-Darwinism make of such cases? Sir Julian Huxley tells us that *very slight selective values can now be seen to be effective, because evolutionary periods are so long.*² But can an incomprehensible happening become comprehensible by lasting a long time? Either one is simply describing the direction of a trend that has already happened, and 'selection' is just shorthand for such a description, rather than an explanatory concept; or 'selection' stands for some mysterious force which controlled the whole process, in Dobzhansky's phrase (to quote another eminent neo-Darwinian), *sub specie aeternitatis*.³ This sounds suspiciously like Schindewolf's foresight of selection, as does Simpson's statement: 'it is certain that if we can see any advantage whatever in a small variation (and sometimes even if we cannot), selection sees more'.⁴

To return to Schindewolf: as against the highly abstract and hypothetical Darwinian theory, he makes the assumption: *that life can originate novelty*.⁵ This is, he says, a preferable assumption because it is simpler. He does not pretend to 'explain' this proposition, and in that sense it may be 'mysterious', but no more mysterious, he says, than physical concepts like 'force' or 'gravitation' which everyone is prepared to accept as 'explanatory'—and not nearly so mysterious, he believes, as the whole nexus of assumptions implicit in the concept of 'natural selection' as the neo-Darwinians use it.⁶

(c) Finally, and most neatly characteristic of this type of situation, the two scientists argue in exactly opposite directions, each taking as his premiss the negation of the other's conclusion. Simpson, wedding palaeontology to the statistical methods of population genetics, sees a gradual change in populations such that the sharp divisions of traditional morphology become false.⁷ Schindewolf, basing his theory on the logical priority of morphology, concludes that the gradualist,

¹ A. Vandel, 'L'Évolution Considérée comme Phénomène de Développement', *Bull. Soc. Zool. de France*, 1954, 79, 341-356. Cf. L. Cuénot, *L'Évolution Biologique*, 1951; 'L'Anti-hasard', *Rev. Scient.*, 1944, 82, 339-346

² In Huxley, Hardy, Ford, *Evolution as a Process*, London, 1954, p. 3

³ T. Dobzhansky, *Genetics and the Origin of Species*, 3rd. edn., New York, 1951, p. 75

⁴ *Major Features of Evolution*, p. 271

⁵ *Grundfragen der Paläontologie*, pp. 425-429

⁶ *Ibid.*, p. 431

⁷ *Major Features of Evolution*, pp. 377-378

statistical picture of neo-Darwinism is false.¹ To put it very schematically; Simpson argues: the neo-Darwinian theory is true; morphology implies that neo-Darwinism is not true; therefore morphology is wrong. Schindewolf argues: morphology must first be accepted as true; morphology implies that the neo-Darwinian theory is wrong; therefore the neo-Darwinian theory is mistaken. Or to put the matter another way, they agree on their major premiss: *traditional morphology and neo-Darwinism are incompatible*. One says: Darwinism; therefore not morphology; the other says: morphology, therefore not Darwinism.

In each book this argument—that is, in effect, the argument on the relative priority of morphology and phylogeny—comes late; but in each book it is a most decisive argument, which has been in fact moulding the shape each theory has taken all along. Why is it so decisive? In neither case has a crucial experiment falsified the other theory. Nor is it merely that the two writers start from different premisses. But these different premisses, leading each to a conclusion directly contradicting the alternative premiss, are symptoms of a more pervasive difference, a difference in ways of thinking. To put it very simply, the two writers look at their material differently. It is this difference in 'looking' that I want to analyse and in part to evaluate.

3 *Planes of Disagreement*

When we consider the two theories a little more closely, we find that the differences between them are themselves of different kinds. Perhaps we may say that our two scientists disagree on different *levels* or *planes* of thought. There is, first, the *verbal* plane. They differ at many points simply in the formulation of observed phenomena or of their generalisations from them. Secondly, there is the *visual* plane. They use different sorts of visual imagery to support their theories; the models on which they rely are different in kind. Third, the plane of *attention*. They pay heed to different aspects of the phenomena. Different *kinds* of question interest them. Finally, and most fundamentally, we come to the *conceptual* plane. Their 'outlook', the interpretive framework *out of which* they are looking at their material, is different.² Here, and indeed on our second and third levels, we will

¹ *Grundfragen der Paläontologie*, pp. 300, 455 et seq., esp. p. 475

² In terms of Professor Polanyi's philosophy of personal knowledge, the two are thinking on opposite sides of a *logical gap*; see M. Polanyi, *Personal Knowledge*, London, 1958, pp. 150-160. In terms of Professor Hodges' Riddell Lectures (*Languages, Standpoints and Attitudes*, Oxford, 1953), they differ *existentially*.

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have moved beyond the range of differences specifiable in terms of vocabulary and syntax, to the more pervasive *tacit* component of theoretical knowledge.¹

4 *The Verbal Plane*

Consider, then, first, the verbal differences between the two theories—those differences which *are* specifiable in terms of vocabulary and syntax. The interesting thing about these differences is that when one looks at them hard, they disappear. It looks as if a great many statements in the two theories could be translated without loss of meaning one into the other. This does not mean that their disagreement is ‘merely verbal’. On the contrary, if their statements were all translated into a common terminology, they would agree verbally but would still disagree.

Let me mention briefly two examples of such verbal disputes. Simpson objects to ‘orthogenesis’ but speaks of ‘essentially rectilinear evolution’,² which is all that Schindewolf’s ‘orthogenesis’ is shorthand for. Schindewolf insists that ‘adaptation and genuine evolution are two quite different things’, yet in speaking of the origin of mammals he says this was a ‘general improvement’ as distinct from the adaptations of particular groups of mammals to particular environments—as the limbs of bats, whales, etc.³ But Simpson also distinguishes between adaptations of a general and variable kind, and more limited and specialised adaptations—like the rodent’s tooth.⁴ Why couldn’t Schindewolf speak of ‘general adaptations’ instead of ‘general improvement’? But he would be very angry at this suggestion, as Simpson would be if told that he might as well say ‘orthogenesis’ instead of ‘essentially rectilinear evolution’.⁵ Obviously the verbal disagreement, which, in these and a number of other cases, could be easily eliminated, is relatively unimportant. It is the difference that would remain in spite of verbal agreement that matters.

¹ See Polanyi, *op. cit.* Part II, ‘The Tacit Component’.

² *Major Features of Evolution*, p. 259

³ ‘Evol. vom Standpunkt eines Paläontologen’, p. 382

⁴ *Major Features of Evolution*, p. 346

⁵ See Simpson’s note, *ibid.*, p. 259: ‘This means the same things *descriptively* as Schindewolf . . . means when he says that orthogenesis is characteristic of the typostatic phase of evolution, although I reject the theoretical implications of his statement as decisively as he rejects mine.’

5 *The Visual Plane*

Let us try, then, to look behind the verbal level of the dispute and consider the differences that underlie it. The two scientists, we said, *look* at the facts differently. They differ in the visual imagery by which their theories are supported. In Simpson's case the visualisation is at a relatively abstract level. Sometimes he uses graphs illustrating

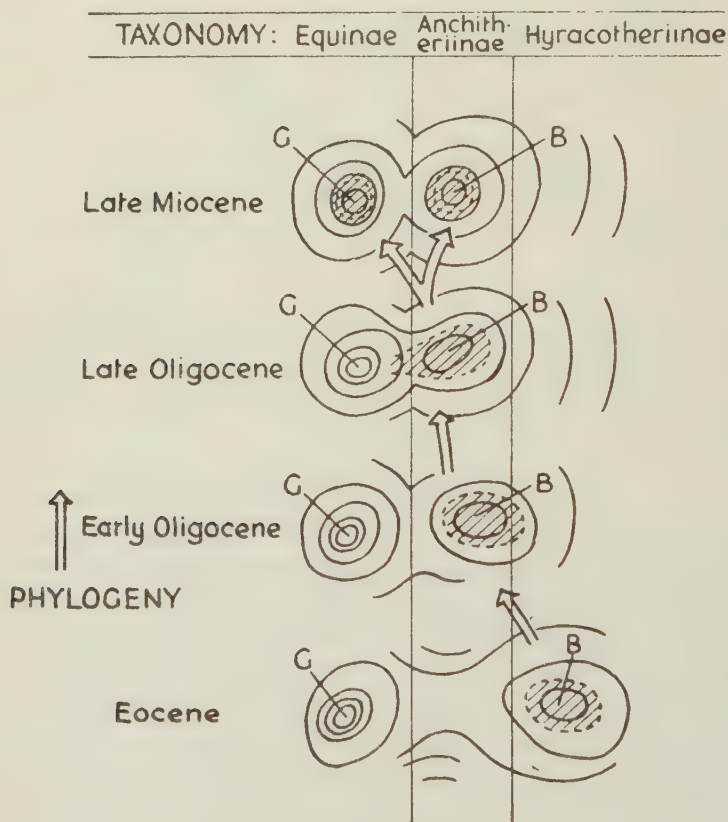


FIG. 1.

complex statistical relationships, sometimes models in the sense of cognitive maps. In Schindewolf's case it is usually, more directly, the actual shape of fossils that we are asked to envisage: these shapes and differences in shape illustrate the relationships characteristic for evolution.

Consider an example from each. Simpson (Fig. 1) summarises the development of browsing and grazing types in equid phylogeny in

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terms of the 'selection landscape' technique introduced by Sewall Wright.¹ Selection landscapes are constructed by applying abstract topological techniques to a wide range of data already subject to elaborate statistical manipulations. The result is a series of 'peaks' and 'valleys', each peak representing an adaptive optimum for the characters and groups in question.² In the case of horse evolution, we have two adaptive peaks, a browsing peak (B) and a grazing peak (G), with periods of shifting between them, and finally (not shown on this diagram) the disappearance of the browsing peak.

Compare with this a diagram of Schindewolf's (Fig. 2)³, showing the development of one type of modern coral (f-h) from an early ontogenetic stage (b) of an ancient coral (a-e). It seems unlikely, if we look at (c) and (h), that a transition between them could have occurred by small cumulative steps. We could imagine, however, that in a young Pterocoral, the third pair of protosepta (c III) failed to develop, and instead (as is the case with modern Heterocorals), the second (f II) and then the first pair (g I) divided laterally, producing the four divisions (and other novelties in the arrangement of the subdivisions) typical of the new kind of animal. This is something one sees intuitively, by looking at the actual shapes of fossil and recent corals at early and later stages of growth.

6 *The Plane of Attention*

The difference in visualisation is symptomatic, further, of a difference of attention. The two are looking at different aspects of their common subject matter. What is central for one is peripheral for the other, and vice versa. Simpson is applying statistical methods in palaeontology, in close reliance on the theories of population genetics, which are also statistical. He is dealing, not with individuals, which do not evolve, but with populations, which *do*. Schindewolf is looking rather at individuals of different shapes and sizes, and the problem is to relate them phylogenetically; this can best be done in reliance on analogies drawn from embryology. From these different perspectives the same 'facts' look different. For example, Schindewolf

¹ Based on *Major Features of Evolution*, p. 157 (Fig. 17). (By permission of the author and Columbia University Press.)

² Selection is represented as positive (uphill) or negative (downhill), its intensity being proportional to the gradient. See *ibid.* pp. 155-157

³ See p. 120. Based on *Grundfragen der Paläontologie*, p. 214 (Fig. 213).

uses the development of artiodactyl and perissodactyl limbs evidence that major steps in evolution occur discontinuously—and that therefore his theory rather than the neo-Darwinian is correct.¹ Simpson cites the development of the artiodactyl limb as a case of what he calls quantum evolution, that is, evolution by an all-or-none

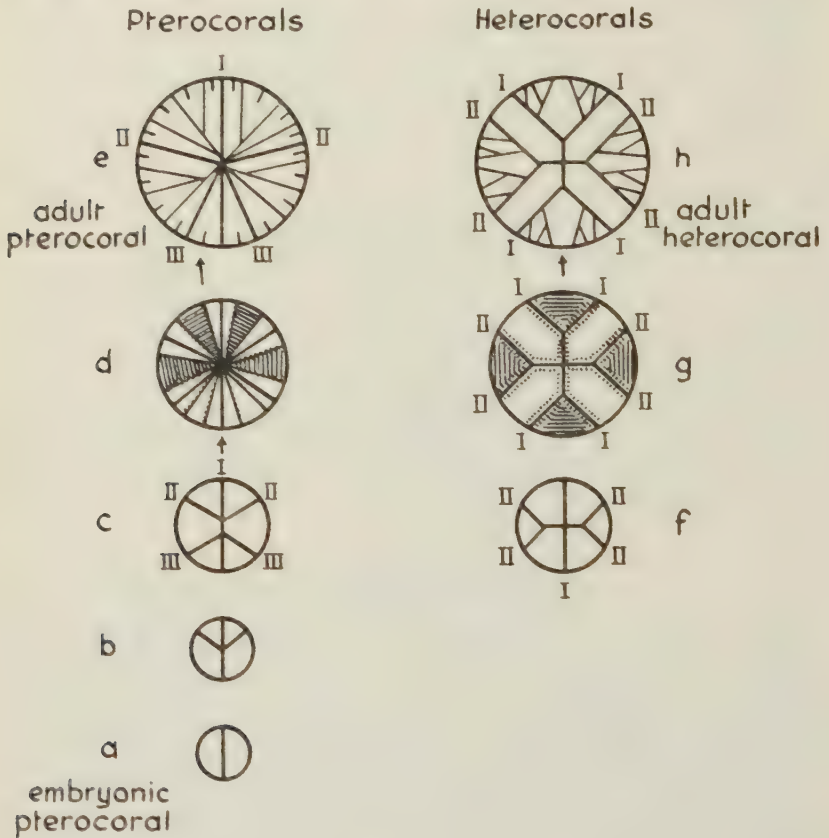


FIG. 2.

transition.² Here the two are even in verbal agreement, for Schindewolf describes his transitions from type to type as 'quantenhaft'. Moreover, both agree that the transition in this case constitutes a sharp dividing line. Yet the different field of attention in each case puts such divergences into a very different context. Simpson is looking at gradual transitions in the genetical make-up of populations, while

¹ Based on *Grundfragen der Paläontologie*, pp. 280-281

² *Major Features of Evolution*, pp. 391-392

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Schindewolf is looking at changes in the structure of individuals, which often vary slightly though sharply, and between two such sharply divided varieties organisms fall easily into 'types'. Thus Simpson sees a sequence of particular changes building up to a generalised trend, Schindewolf a general pattern with lesser changes within it.

Let us look more closely at Simpson's account. Populations, from the point of view of population genetics, are aggregates, not even of individual organisms, but of gene-complexes, tending to vary this way or that. A population is conceived in terms of its 'gene pool', that is, the sum total of the genes of its members' germ cells. 'Genetical selection' is the concept used to describe the systematic direction of the variation of genes, as distinct from drift, a minor factor, chiefly in small populations, which consists in fluctuations that fail to establish well-defined trends. Concepts such as 'threshold' or 'break-through' serve to facilitate the understanding of relatively abrupt shifts in such gradually changing trends; but what is of primary interest is the gradual change in the overall distribution of genes in interbreeding populations. That there was a first *Eohippus* or *Mesohippus* or *Equus* is not nearly so interesting as the fact that there were gradually more.

Attention to overall population trends, moreover, is supported by attention to the shifting organism/environment relationships—strictly speaking, gene-pool/environment relationships—which underlie them. Individual wholes are not of interest in this vision of pattern growing out of flux. And of course, it is primarily the 'mechanism' of these innumerable small changes that population genetics has so successfully interpreted. Minute alterations in the material of heredity do tend to appear in experimental populations, and to perpetuate themselves. Such systematic change in hereditary material is called 'genetical selection', and selection is said to be the 'mechanism' by which such change happens. It is not, however, a 'mechanism' in terms of a cause-and-effect relationship, for the occurrences it correlates are random. Nor are internal laws of structure or development of interest to this kind of evolutionary thinking. Selection in the sense of population genetics is simply any systematic change, regardless of cause, in the make-up of the gene-pool of an actively interbreeding population. The occurrence of mutations being taken as random, selection is the settling of these random occurrences into statistically manageable averages. It is represented by the curve established by reading averages of gene frequency over a number of generations. Except in

direct relation to reproduction, therefore, selection, in the sense of genetical selection, has no *intrinsic* relation even to adaptation. Simpson himself makes this quite plain. That such systematic change produces adaptation can be stated only on empirical evidence, or as a consequence of relaxing the strict genetical definition.¹

Schindewolf, on the other hand, is looking at the difference, say, in the shells of cephalopods or the structure of corals. The interesting point is not that the number of ammonites or of heterocorals came gradually to a peak, but that there were first ones, abruptly different from anything appearing in lower layers of fossil deposits. These changes cannot be interpreted as crystallising out of fluid gene-pool/environment relationships, since they often appear when there is no evidence of significant environmental change. In fact, in Schindewolf's view, it is by the change in fossils that the succession of strata must be ordered, not vice versa. Moreover, the new types often show no perceptible difference in adaptive relations. In the history of corals, the change from six to four divisions is not a change in adaptation. It is just a change in plan. What we are looking at is a set of forms, macroscopic structures, *Gestalten*, which are in their nature discrete. The problem, for example, for the two types of coral, is to account for the continuity between them: and here the concept of early embryonic change (proterogenesis, or in English usage pedomorphosis) comes to our aid. Such change is also mentioned by Simpson, but concerns him little; for what interests him is the statistical trends in *populations*, not the life history of the individual embryo. Thus, though it is Simpson who seems to have moved beyond a succession of stable fossil forms to the genetic basis of evolution, it is Schindewolf who sees these stable forms as relics of living individuals and connects them, in imagination, through concrete, developmental stages rather than through abstract, statistical change. It is true that populations, not individuals, evolve; but populations are populations of individuals,

¹ Dobzhansky, *op. cit.* pp. 79-80, equates 'adaptive value' with 'differential reproduction' (i.e. with genetical selection), thus concealing from the start the fact that the two are not conceptually equivalent. In this connection it is interesting that genetical selection is so often called the only known 'mechanism' of evolution, when it is by definition not a mechanism at all, but a statistically established trend. (See e.g. *Major Features of Evolution*, pp. 144-146). Strictly speaking, the 'mechanism' resides in the mathematical skill of scientists like Fisher, Haldane, Wright and Simpson himself. It is only in the older and looser Darwinian sense that selection is genuinely a mechanism in nature. Dobzhansky's definition glosses over this difficulty; Simpson admits it at the start but tends to forget his admission.

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and something that happened in individual development must have underlain the abstract statistical relationships which population geneticists construct. It is this underlying *real* change that Schindewolf is trying to envisage. In other words, where Simpson interprets the palaeontological data in conjunction with the abstract generalisations of population genetics, Schindewolf relies on the embryologist's conception of individual development.¹

In short, looking at populations throughout evolutionary history, we see a series of ever-branching particular directions establishing relatively stable interplay of gene-complex/environment relations. Both these variables change continuously, and so does the result, statistically viewed, of the outcome of their interactions. Looking at individuals, we see a multiplicity of types, which, once there, adapt themselves to particular variations in environment, and of course die when they become mal-adapted; but the initial divergence in types is the fundamental, and irreducible, starting-point. For Simpson, evolutionary change consists in the 'break-through' of a population into a new 'zone'. This may occasionally happen as an all or none reaction, but not generally. The so-called explosive phase of evolution is usually just a speed-up of normal speciation 'due' to intensive selection pressure in a new 'zone'. For Schindewolf, evolutionary change occurs when a youthful animal takes a new turning on its ontogenetic course, and so, as Hardy puts it, 'escapes from specialization',² with the result that something new—a new ground plan—appears which is withdrawn momentarily from the confining influence of selection, and proliferates, out of sheer exuberance, in all sorts of directions.

7 *The Conceptual Plane*

Our two scientists, then, not only lean on different kinds of visual images as models, but look at different aspects of the phenomena. Why these differences? Why do they use such different visual aids? Why is their attention so differently directed? The answer can be

¹ In this connection see the important essays of A. Dalcq on ontomutations, 'L'Apport de l'embryologie causale au problème de l'évolution', *Port. Acta. Biol. Vol. Jub. Goldschmidt*, Coimbra, 1949, pp. 367-400, and 'Les Ontomutations a l'origine des mammifères', *Bull. de la Soc. Zool. de France*, 1954, 79, 240-255; also A. Vandel, *op. cit.*

² Sir Alistair Hardy in Huxley, Hardy, Ford, *op. cit.* pp. 122 et. seq.

given only out of a third sense of 'looking': what they are looking *for*, the general perspective out of which they look, is different. They get different answers from nature, in Kantian terms, because they are putting different questions: and they put different questions because they stand at different places in relation to their subject-matter: their 'outlook' is different. This may sound like playing with words: one can only see what one is looking at, and one only looks at what one is looking for. But consider our two cases in terms of this question: what are they looking *for*? What is their outlook, their way of seeing? That is really the most fundamental kind of difference, on which the differences in verbal usage, in visualisation, and in attention, all depend.

Clearly, as I said at the beginning, Darwinism (and following it, neo-Darwinism) takes its stand firmly on the ground of continuity.¹ It sees life, and ultimately matter, as continuous and therefore explicable, in the last analysis, in terms of a *single* set of principles. Schindewolf's sort of theory envisages a variety of types, a set of discrete wholes or patterns, in step-like arrangement. This simple alternative may seem to be contradicted by the statement of Dobzhansky (certainly among the most authoritative neo-Darwinians) that the most striking phenomenon calling for explanation by the biologist is *discontinuity*.² But actually Dobzhansky's statement confirms my interpretation: continuity is the fundamental framework within which neo-Darwinian biologists are thinking, and discontinuity is the surprise they must explain away. They have to show how out of continuity the actual existent discontinuity—the biological discreteness of species, genera, etc.—could have arisen. Schindewolf, starting with the discreteness of different morphological types, has to explain their continuity. For him the panorama of forms comes first, and since these do appear, in a series of strata, with increasingly complex novelties succeeding one another, it becomes necessary to explain how transitions could have happened, as Schindewolf certainly agrees they did, between such basically different forms. Hence, as we have seen, his interest in early embryological change, for in this kind of change one can envisage an abrupt alteration in structure which nevertheless takes account of the actual continuity in the successive generations of living things.

Their disagreement about the relative priority of morphology and phylogeny lies close to the heart of the matter. Schindewolf looks

¹ This is not, of course, a mathematical continuum, but a series of minute changes conceived as functions of continuously changing particulars.

² T. Dobzhansky, *op. cit.* pp. 3-4

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first at structure, and can't understand how anyone can look at phylogenetic relations without having first looked at structure. But the great vision of Darwinism is precisely the dissolution of stable forms into a continuum. What Darwin did was to unsettle the fixity of species, to make nature flow. If he threw Paley's watch into the middle of the ocean, life itself became that ocean; in Simpson's image of selection landscape: 'rising, falling, merging, separating, moving laterally, at times more like a choppy sea than like a static landscape'.¹ And once life is seen as flux, change in its 'forms' is reduced to nearly imperceptible transition, the gradual elongation of the elephant's trunk or the gradual disappearance of the equid's extra toes, and so on.

If, however, the stress on continuity is essential for neo-Darwinism, that conception alone is by no means definitive for the neo-Darwinian outlook. A Lamarckian theory would also stress continuity, through the gradual establishment of new techniques to meet new needs. A Lamarckian theory would also, of course, stress adaptation, which the Darwinian view, as against Schindewolf's, also emphasises. What differentiates the neo-Darwinian theory is the peculiar combination of three basic concepts: continuity, particulate inheritance (and mutation), and adaptation. What is this peculiar combination? How do these three ideas hang together in the neo-Darwinian conception of evolution?

First, let us examine the relationship between the particulate mechanisms of heredity and changes in heredity. This at first sight looks incompatible with the emphasis on continuity and gradualness of change and, of course, mutationism was for a while thought to contradict the basic Darwinian view. But as Fisher declared twenty-five years ago,² the knowledge that mutations are the bearers of evolutionary change in fact proves the triumphant support, the very keystone, of neo-Darwinism, and the want of this knowledge was precisely what made Darwin's own position sometimes inconsistent. How, conceptually, do continuity and the particulate character of hereditary material go together? In two ways. First, to think in terms of small building blocks, like genes, is to think in terms of parts, not wholes, or of populations of such parts, that is, of collections of particulars, which again are not wholes. Moreover, as we have seen, the populations of population genetics are not even collections of whole

¹ *Major Features of Evolution*, p. 157

² Sir Ronald Fisher, 'The Bearing of Genetics on Theories of Evolution', *Science Progress*, 1932, 27, 273-287

organisms, but collections of gene complexes, and a gene complex, again, is a sum of independent particulars. Even though its *effect* be conceived as integrated, in its constitution it is of a mosaic rather than a comprehensive kind. It is not an organic whole, but a juxtaposition of small discrete particulars. In short, both types of concept: continuous variation or change, and particulate inheritance with its minute though discrete alterations are, precisely, devices for *not* looking at wholes or Gestalten or structures: for reducing organic structure, on the one hand, to values of continuous functions, and on the other, to collections of minute particulars. Here, I believe, we really meet the ruling passion of Darwinism: in the determination not to look at structure. Structure must be explained *away*; it must be reduced to the conditions out of which it arose rather than acknowledged *as* structure in itself.

This first part of the answer is negative, and admittedly does not take us far, since particulate pattern and continuity are so obviously discordant conceptions. The second part, however, is positive: this is where population genetics steps in to reconcile the discrete and the continuous. It is a most powerful instrument for synthesis since, statistically handled, collections of genes, which are particulate, do nevertheless vary continuously. Combined with Mendelian statistics—which are themselves grounded in a particulate, non-structural view of the nature of organisms—the statistical methods of population genetics allow one, therefore, to view the fluctuations in Mendelian statistics (which appear as the changing average of the relative frequency of one gene over its alternative in interbreeding populations) as producing a continuous process of evolutionary change. Nor is that all. Statistical calculations of rates of evolution, the construction of adaptive peaks and valleys, etc., which are included in the apparatus of population genetics, allow one to construct a highly abstract quasi-structure which appears to introduce a satisfactory—because abstract—pattern into an otherwise unintelligible flux.

What about adaptation? Again, the central role assigned this concept does not follow from the concept of continuity in itself. Leibniz saw the world as a continuous array of degrees of perception, not of adaptations. In fact, it seems odd that the very theorists who most emphatically deny any shred of purposiveness to nature, should just as emphatically declare that all significant changes in nature are adaptive in character. But the stress on adaptation falls into place once one takes the refusal to look directly at structure as fundamental. It is the shifting organism/environment relation, not the form of the

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organism itself, that is the basic unit. Organisms, already dissolved, from the perspective of evolution, into gene complexes, are themselves constantly changing as an (equally changing) environment plays upon them. Thus what changes is itself a product of two variables: average gene frequency and environment. There are no constants which would have to be assessed as patterns or achievements in themselves, only what Simpson calls the 'splendid opportunism' of life.

This is indeed a brilliant perpetuation of Darwin's vision, and its persuasive force is compelling. Schindewolf's principles are simpler. He sees typical shapes, and he sees again and again what appear to be new shapes. Therefore he assumes that living things are able to originate novel types. Mutation, he agrees, must have been the mechanism by which they originated; but the adaptive control of mutation occurs only within, not between types. The basic pattern is of change from type to type, and always, as we have seen, with the more general appearing before its specialised subdivisions. We cannot, in fact, describe specialisation except *within* a more general type: there must be some kind to become specialised.¹ First we have types, then we acknowledge the origination of new types. Within these, there is an inner dynamic of fulfilment: orthogenesis, and at the same time radiation into specialised subtypes controlled by adaptation and selection—the last and least creative steps, leading each time to an evolutionary cul-de-sac, whether as with *Lingula* to arrested evolution or, as with the majority of species, to extinction.

(to be concluded)

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¹ Simpson's argument that the higher categories are adaptive does in fact generally involve reference to a group which separates out *within* a still wider group—as bats, or rodents, or carnivores versus ungulates, and so on. See Chapter XI, *passim*, esp. pp. 346 et seq.

NOTE AND COMMENT

A Note on 'Theories, Dictionaries, and Observation'

In my article¹ I overlooked some special circumstances under which a formal theory which is translated by means of an arbitrary dictionary into phenomenal statements may be said to be testable. Suppose the dictionary consists of n entries translating expressions of the theory into concepts $\alpha_1, \dots, \alpha_n$, which may be regarded as being operationally defined in the phenomenal language. Then there may be $(n - 1)$ independent functional relations between the α 's, all of which are directly testable. It may happen that a formal theory is constructed after only some of these have been discovered; it is then possible to say that the deduction and translation of the finite number of such relations remaining enables the theory-plus-dictionary to be tested.² Such tests are distinguished from others by requiring no new entries in the dictionary, and I shall call them *formal tests*.

If the phenomenal language is defined in such a way that it does not contain any concepts which imply correlations or connections not assumed in non-scientific discourse, then formal tests are usually, but not always, trivial. Most *applications* of a theory will be formal tests, for example if operational definitions are given of the time and space coordinates of astronomical objects, then the prediction of the orbit of a comet or an earth satellite is a formal test of Newtonian gravitation, and this is more usually regarded as an application than a test. But Newton's comparison of the force required to keep the moon in its orbit with the gravitational force at the surface of the earth was not a formal test, because his *phenomenal* language did not include a concept of force referring indifferently to celestial and terrestrial dynamics. On the other hand, Hertz's experiments on the transmission of electromagnetic waves were formal tests of Maxwell's equations, since they required no new operational definitions in the phenomenal language, and as a matter of historical fact these tests were not trivial. If, however, the formalism of Maxwell's equations had been known before the properties of magnetic

¹ This *Journal*, 1958, 9, 12

² This is implicit in Professor Braithwaite's account of the 'four-factor' theory (*Scientific Explanation*, p. 68 ff). I should not have ascribed to Professors Braithwaite and Woodger point (d) on p. 14 of my article, since their theoretical statements do not constitute a distinct language, but merely contain theoretical concepts which are eliminated in the deduction of phenomenal statements. But my argument still shows that these theoretical statements cannot, in general, be tested.

needles (as might very well have been the case if electric currents had been discovered before magnetism), a prediction derived from Maxwell's theory about the behaviour of natural magnets would not have been formal, for the necessary interpretation of the theory would have involved some electronic model of matter and new operational definitions enabling the theoretical term 'magnet' to be interpreted as descriptive of actual bodies having a certain chemical constitution. Other, non-fictional, examples of non-formal tests were given in my paper.

If, however, the language into which a theory is interpreted for test purposes is the entire language of classical physics, as is the case for quantum theory, then formal tests may be more interesting and important. The situation here is somewhat obscured by the fact that quantum-mechanical formalisms have evolved by close analogy with classical theory, and hence it appears artificial to consider them as different languages and to suggest that any 'dictionary' between them is required. When a new formalism is introduced in quantum theory the immediate problem is to find its 'physical interpretation', and by this is meant an interpretation in specifically quantum language which while not phenomenal nor even classical, yet shows by the model which it exhibits what classical test-statements will be relevant to it. The whole procedure of quantum theory, with its insistence on physical interpretations, is in fact a refutation of the formal view of theories. But whether any particular prediction, for example of the properties of a new particle, constitutes a formal test or not, will depend on whether the formalism from which it is derived is already wholly interpreted in classical terms or whether it contains some symbols whose classical interpretation is not known. The discovery of the positron, for example, was a non-formal test, since it involved the new hole-theory interpretation of a quantum-mechanical formalism which had been *formally refuted* by the non-appearance of particles of negative energy.

MARY B. HESSE

DISCUSSIONS

WHAT MAKES A SUBJECT SCIENTIFIC?

W. B. GALLIE's paper in this *Journal*¹ under the above title does not expose an ambiguity in the question itself, which may represent an inquiry as to whether there is anything about a subject ('whether in its nature, its treatment, or otherwise) that marks it decisively as scientific; or may ask simply what it is that makes a potentially scientific subject an actual scientific subject in the sense that it qualifies for inclusion in the conventionally recognised scientific tradition or discipline. The importance of drawing attention to the second and subsidiary question lies in the danger that the answer to it (which is easy to find and quite irrelevant to the substantial question) may be offered as a solution to the problem proposed. The paper referred to does not appear to have avoided this trap.

It will help to clear the ground if this secondary question is disposed of, and this is most readily done by taking a concrete example. Farming is a potentially scientific subject, but as studied by the average farmhand it hardly qualifies for recognition as a scientific subject in the secondary sense. To rank as agricultural science it must be studied systematically upon the basis of a sufficiently elaborated and ordered public record or tradition of information and opinion. Just how systematic the study must be, and how well-developed a record is required to effect the transition, is a matter, not for definition, but for agreement. A conventional standard must be established as a yard-stick and even then, there will be room for disagreement as to whether the subject as studied measures up sufficiently to qualify for recognition. It is plainly misconceived to attempt to frame a definition of a scientific subject that would exclude farming from agricultural science. Nevertheless, the demand for a standard of study on the basis of a reasonably established public record is apt to give the impression that the scientific character of a subject is in some way related to publicity, tradition, and history. It may also lead to the suggestion that such matters as communicability, verifiability of data, and a high degree of systemisation are *indicia* of science. The fallacy lies in the fact that all the requirements mooted along this line apply in equal force in every discipline. Thus, the legal discipline emerged with the reporting of case-law, the promulgation of statutes, and the issue of treatises designed to establish a systematic order out of the information so gathered. A subject can only be recognised as a legal subject, in the secondary sense, if it is systematically studied by reference to the tradition established by that means. The fact that a subject forms a well-recognised part of either the legal or scientific disciplines, and is rooted in the appropriate historical tradition, may be very cogent evidence that the subject is legal or scientific (as the case may be); but no light is thrown thereby upon why the discipline is legal or scientific, and we still do not know why the subject has been drawn into the one rather than into the other. It is difficult to resist the conclusion that the scientific character of a subject does not depend upon its forming a part of the scientific discipline but that it falls into that discipline because it has a certain character that we

¹ 1957, 8, 118 et seq.

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deem, for reasons we cannot identify, as scientific. The substantial question is thus left isolated, and a number of convenient argumentative bolt-holes have been closed.

This is not the place to review the *indicia* that have been proposed to distinguish scientific subjects or activity, nor is it necessary in view of the lucid and admirable paper that has prompted these comments. It may be permissible, however, for a lawyer to propose a series of interconnected definitions that result from an attempt to mark out the frontier between science and law in the no-man's-land of psychology. It is recognised that, in the form proposed, these definitions are too gross for acceptance by scientists as they stand, but reasons will be advanced for supposing that they may point in a direction worth further consideration.

1. *The province of science* is the whole area of human interest that can profitably be studied without the need to assume any factor of responsibility in the subject-matter of study to account for its condition or behaviour ;
2. *The scientific method* is any method that can profitably be employed without making the assumption of responsibility referred to ;
3. *A scientific subject* is one that (a) is amenable to study in accordance with the scientific method and (in the secondary sense) (b) is studied in accordance therewith sufficiently methodically by reference to an adequately developed public record of information and opinion ;
4. *Science* is the body of recorded information and opinion that represents at any given time the public tradition arrived at by the study of scientific subjects in accordance with the scientific method.

From the scientist's point of view, the keyword of this series of definitions, namely responsibility, is so dangerously loose and indeterminate that their value must, at first sight, appear dubious to say the least. Nor is it easy to find a satisfactory substitute. Were it not for the doubtful uses to which the term, subjectivity, is put, this might be preferred. But even if no satisfactory term is found, it is not difficult to appreciate the crucial factor which the scientist either does not find or, for the purposes of his study, declines to employ as a methodological instrument. Whatever it may be called, it represents a certain liability to criticism or approval. The scientist does not catch himself out saying to the subject-matter of study: 'Good effort ! One more trial and you will pull off what is expected.' The responsibility-factor attaches to himself and to his colleagues, and not in the least degree to the subject studied. This factor might, perhaps, be regarded as a capacity (however small) to select the response to experimental interference.

If the scientist is content, despite the obvious difficulties, to accept the definitions provisionally to see what clarifications, if any, they achieve, he may, even though still dissatisfied, deem it useful to explore the general direction in which they point, namely, the nature of the methodological *assumptions* employed by scientists.

The definitions plausibly suggest that there is a certain unique economy in working assumptions in scientific method that produces what we call 'science'. This enables us to see precisely why it was necessary for the animistic assumption to wither so that science could take root in its primitive forms; and it explains also, why scientists at a much later date were obliged to resist the introduction of God as a methodological instrument of explanation despite the blandishments and threats of theologians who were alarmed lest this scientific license should lead to the outright rejection of God as a factor to facilitate explanation in other fields of human interest. We can see more

clearly, too, why physics, chemistry, biology, and allied subjects are regarded as the classical province of science, because no one but a madman or an animist would wish to introduce the responsibility-factor into that field. The problem as to whether psychology ought to be accepted as a science in the strict sense is, likewise, illuminated, for here is a field of interest in which the subject-matter can be profitably studied without recourse to the responsibility-factor, but that factor can be, and frequently is, adduced as a working assumption at a certain stage of the inquiry (e.g. in psychiatry). Whether a mixed discipline of this sort should be regarded as a science, or split into a strictly scientific and an extra-scientific component, is a matter upon which opinion is likely to be divided.

Similarly, light is thrown upon the common distinction between 'pure' science and the engineering sciences. An engineering science is involved in the study of situations that include a subjective factor, e.g. somebody wants to reduce the pressure of traffic by building a new bridge. The engineer must adapt his specification to take account of this subjective element; but, once the specification is approved, he proceeds on strictly scientific lines to produce the bridge specified, and he is in no danger of regarding the stresses and strains affecting the structure as subjective in the same sense as are the stresses and strains that affect the motoring public in the absence of his bridge.

The definitions clear up other problems. It becomes apparent that the scientific method is not limited in its applicability to the scientific range of subjects, for it is of the greatest utility in disciplines that are not sciences. History, economics, law, and all the other disciplines fall back on the efficient economy of the scientific method whenever it is apt to the task in hand; but this does not imply that these disciplines are scientific, because the public record or tradition upon which they rest is constructed squarely on the assumption of responsibility.

Finally, the definitions offer an interesting justification for, and a decisive criticism of, the persistently advanced theory that there is something uniquely objective about scientific work. This theory mistakenly applies the criterion to the scientist himself, who is supposed to enjoy a cold, emotion-free, impartial objectivity that sets him apart from the artist, the religious adept and the lawyer. The definitions, however, leave the scientist free to obtain his inspiration by whistling in his bath like any other normal man, and only forbid him to impute a subjective factor to the subject-matter of his study as a means of accounting for its condition or behaviour. In that severely restricted sense science is a discipline that 'excludes subjectivity'. Moreover, we can now find the true ground for the persistent feeling that there is something unique about science and its method (as opposed to its methods—which are apt to turn up in the most unlikely extra-scientific places) and the equally persistent feeling that science enjoys a unique efficiency that is related to a unique economy of method.

The definitions thus tentatively proposed are advanced to be superseded. Their utility appears to lie in drawing attention to the possibility of defining science and scientific activity in terms of the working assumptions employed as compared with those employed in other fields. The crucial point appears to lie in the question whether the rational processes of the student are working by reference to an assumption of subjectivity (however named) or not, and it follows that a subject, to be scientific, must be amenable to rationalisation without recourse to that assumption.

SETON POLLOCK

THE SCOPE OF QUANTUM MECHANICS

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IN a recent adverse review¹ of Dr David Bohm's *Causality and Chance in Modern Physics*² by Professor L. Rosenfeld, the following statement occurs: 'The true character of the causal relations of atomic physics was fully elucidated by Bohr and Heisenberg³ thirty years ago. . . .'

Call this statement 'A'.

A suggests an unwarranted precision. Exact science can never correctly define the limitations to the validity of its most comprehensive or advanced expressions, and individual scientists never know the boundaries of their own knowledge. For, as is now well understood, the limits (in scope and accuracy) of a theory, T_1 , can only be defined in the terms and calculus of a more general theory, T_2 . If T_1 is the Quantum Mechanics (Q.M.) of 1925-28, T_2 still lies ahead. Unqualified claims on behalf of Q.M. to full coverage of some specified realm may therefore be disproven at any time in the future by a fundamental advance, whose character—if history provides any precedent—is likely to be unexpected.

In order to emphasise the need for caution it may be useful to list a number of overlapping problems in relation to some or all of which (or perhaps to special cases of these problems) it may prove that the canonical methods of Q.M. are necessarily at some point inadequate, either by failing to provide an effective approximative procedure or in some more radical manner:—

- (i) *High energy* processes (including nuclear stability and transformations, variety of particles, etc.).
- (ii) Processes where the *finite values of certain dimensionless constants* render established methods ineffective (some particle interactions).
- (iii) *Derivation of the dimensionless constants* of current theory.
- (iv) Processes possibly requiring more powerful *relativistic methods*, or the equivalent.
- (v) Certain relatively low energy transfers in *complex systems* (low temperature properties, electron plasmas, etc.).
- (vi) The three-dimensional representation of the *changing inter-nuclear relations* (detailed paths of nuclei) during chemical reactions.
- (vii) The determination of the *equilibrium and changing angles* in complex atomic and molecular systems.
- (viii) Certain *chiral* (left- and right-handed) *properties*, where current theories are awkward or obscure (particle parities, non-symmetric strain tensors in crystals, rotatory power of dispersions of molecules in various media, etc.).

¹ *Nature*, 1958, **181**, 658

² London, 1957. Several favourable reviews have appeared elsewhere. An objective analysis of the De Broglie-Bohm-Takabayasi theory has been made by H. Freistadt, 'The Causal Formulation of Quantum Mechanics of Particles' *Suppl. al Vol. V (Series 10) del Nuovo Cimento*, No. 1, 1957, 1-70.

³ Max Born is not mentioned, though his suggestion, based on much earlier ideas of Einstein, of the probabilistic interpretation of the wave-function was crucial. See W. Heisenberg in *Niels Bohr and the Development of Physics*, London, 1955, p. 13 and M. Born, *Science*, 1955, **122**, 675.

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This list of realms in which boundaries to Q.M. may conceivably be found is intended to suggest that the limitations of Q.M. may not lie only in the field of 'high energy processes', as now understood. It would, indeed, be surprising if the concepts and principles of Q.M. could validly suggest where they are invalid.

Now reconsider *A*. The valid content of *A* can, it seems, be no more than a (concealed) definition of what the writer chooses to call 'atomic physics'. But if this was the intention, it is surely unfortunate, since:

1. A common usage treats nuclear physics as a branch of atomic physics.
2. The identification of (a) 'atomic physics', with (b) 'the valid scope of Q.M.', is not only vague, since (b) cannot be defined today, but will involve the abdication of atomic physics from all claim to be the fundamental exact science, if any realms now obscure prove to lie outside the scope of Q.M.
3. The proposed usage is etymologically inappropriate. The search for the basic 'atomic' (particulate) structure of physical systems continues in the high energy and other obscure realms.

If the above reasoning is in some degree valid, the reviewer's scornful manner is robbed of any excuse. Perhaps for the benefit of the readers of *Nature* he will explain what precisely he meant by 'atomic physics', and define the realm in which he considers that the methods of Q.M. are valid.

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WITTGENSTEIN'S REMARKS ON THE FOUNDATIONS OF MATHEMATICS

I Introduction

It will be convenient to distinguish in this review ¹ between (i) the *foundations of mathematics* or *philosophy of mathematics* and (ii) *general philosophy* in the following way: the former applies to philosophically interesting differences between various parts or aspects of mathematics, the latter, in so far as it is concerned with mathematics, applies to philosophically interesting differences between mathematics and other intellectual activities. For instance, the questions 'what is a (correct) proof' or, generally, 'what is mathematics' belong to general philosophy, 'what is a constructive proof' or 'what is a predicative concept' belong to the philosophy of mathematics.

Wittgenstein's remarks are concerned both with general philosophy and with the philosophy of mathematics. The vivid and incisive language (of the original ²) and the many stimulating questions apart, they make a mixed impression. The aim of this review is to draw attention to observations which seem novel and stimulating, and to bring out their limitations. More space is devoted to the latter because a reader who is interested in the topics mentioned, will turn to the book itself for details.

I.1. In my opinion, Wittgenstein's significant contributions to the philosophy of mathematics concern very elementary computations, a subject which seems to have been neglected by contemporary philosophers though not e.g. by Kant. Also Wittgenstein emphasises repeatedly those aspects of proofs which are neglected in the customary treatment by the methods of mathematical logic. These matters are described in sections 7 and 8 below. His most striking fault is that he believed that all significant philosophical problems occur at the level of elementary computations, and that he made unwarranted generalisations from this limited region of mathematics to mathematics generally. But even if one is aware of other philosophical problems, the book gives a misleading impression, because it suggests

¹ L. Wittgenstein, *Remarks on the Foundations of Mathematics*, Basil Blackwell, Oxford, 1956, pp. xix + 196, 37s. 6d. The references in the review are to the German text.

² L. Wittgenstein, *Bemerkungen über die Grundlagen der Mathematik*. Miss G. E. M. Anscombe has asked to publish some corrections to her translation. These will be found at the end of this review.

an artificial dichotomy in the foundations of mathematics, so to speak: Wittgenstein against the rest. In fact, the aspects emphasised by him are a few among many. To show this, and to put his observations in perspective, we give, in sections 5 and 6, some rough indications of research in the foundations of mathematics. Naturally, large portions are only indirectly connected with the book since their purpose is to indicate what relevant material was left out of consideration altogether by Wittgenstein.

1.1a. In sections 9–12, I discuss a number of Wittgenstein's comments on 'higher mathematics' which are, for the most part, uninformed. While it would be foolish to dismiss the book because of the poor quality of these comments, it seems desirable to deal with them to keep the record straight.

1.2. However, the bulk of the remarks is concerned with general philosophy. To give some sort of intelligible account, it is necessary to discover general themes to which these remarks are relevant. I believe there are three:

1.2a. A theorem is a rule of language and the proof tells us how to use the rule (p. 81, 39).

1.2b. The meaning of a theorem is determined only after the proof (p. 79, 31 or p. 122, 30).

1.2c. A calculation is a (psychological) experiment (p. 99, 76).

1.2d. Further, I believe the link between these themes is that they are naturally suggested by an empiricist's approach to the foundations of mathematics: I am not concerned with discussing different brands of empiricism, but with the general tendency of discussing a subject in terms of things perceived and done, in short, of facts and phenomena (not in contrast to real objects, but to introspective analysis and sophisticated abstractions used in the advanced sciences).

Wittgenstein does not accept the doctrines 1.2a–1.2c; 1.2a is criticised, e.g. on page 119, 18; 1.2b on page 77, 27, or page 165; and 1.2c is rejected, e.g. page 95, 67. Generally speaking, the fault lies in the assumption (of naive empiricism) that there is a sharp distinction between the empirically given and the means of description, while according to Wittgenstein (p. 173) we need concepts to tell us what are the facts. These matters are described in section 2.

N.B. In the sections concerned with general philosophy I go by impressions and quotations out of context. Perhaps a professional philosopher will examine my case on the basis of more substantial research than I have put into this review. Also it should be observed that, below, Part I of the book is barely considered because it is closely related to Wittgenstein's *Philosophical Investigations* (for an exception see, e.g. p. 11, 27).

1.3. What has been described so far is not at all like the popular impression of 'Wittgenstein's philosophy' such as his anti-metaphysics, his panaceas of *rule of language* and *application*, his attitude

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to traditional schools of philosophy. Certainly, the book is not free from these traits of Wittgenstein's writings: but I do not believe that they are of any significance, and, in point of fact, when he embarks on serious analysis such as mentioned in 1.1 (and in sections 7 and 8 below), there is no trace of them.

1.3a. Consider the argument against the notions of mathematical *object*, *necessity*, *truth*, (e.g. p. 60, 10 or p. 142, 16): they are supposed to be metaphysical and to get in the way of straight thinking like alchemy.¹ Even if this is granted, what about the reduction to rules of language or rules of grammar? A grammatical assertion such as "soon" is an adverb' is established by an argument, it is not a simple choice once the notion of an adverb has been introduced, and similar problems arise here as with mathematics. And when one really does not understand the content of a mathematical proof or assertion, one does not know what to count as its application (p. 165, line 3) or even where to look for it (cf. 6.3 below). The effect of Wittgenstein's use of 'application' is really to exclude introspective analysis, and I am not convinced that this is right in the problems which interest him specially (e.g. p. 120, 26).

1.3b. He regarded the traditional aims of philosophy, in particular of crude empiricism, as unattainable. He objected to a mathematical foundation of mathematics because the concepts used in the foundation are not sufficiently different from the concepts described (p. 171, 13) and, he thought (p. 177) that there are no mathematical solutions to his problems. He said (p. 171, 13) that the aim of a philosophy of mathematics should consist in a clarification of its grammar (though I have not found a single passage in the book which I could with certainty identify as a 'clarification of grammar').

These general conclusions are criticised in sections 3 and 4.

1.4. I give a table of contents of this review to help those readers who get lost among the many and diverse topics which have to be mentioned if an even remotely adequate idea of the content of this book is to be conveyed.

General Philosophy

	Sections
Limitations of Empiricism	2
<i>Philosophy of Mathematics</i>	
Criticism of Wittgenstein's general views	3-4
Background material	5-6

¹ Another objection cites the misleading (mystifying) pictures that may be associated with these notions (e.g. p. 36, 119). Equally superficial objections can be made to 'rule of language': is it made by a secret body? Is it renewed?

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Philosophy of Mathematics (cont.)

Strict Finitism	7
Proof Theory	8
Specific topics in higher mathematics	9-12
I also add a personal note	13

2 Limitations of Empiricism

The present section has the modest aim of suggesting a framework for reading Wittgenstein's remarks on general philosophy. For lack of a true understanding of the matters involved, I shall employ concepts commonly used in philosophy, by Wittgenstein and others, although I know how superficial similar-sounding concepts are when applied to the subject matter about which I have thought, e.g. sections 5-8 below.

Wittgenstein's starting point is this: he is not prepared to use the notions of mathematical object and mathematical truth as tools in philosophy. Actually he gives some arguments against them, but, as stated in 1.3*a*, I do not find them convincing. To me the real objection to these notions is that, at any rate as far as I know, there does not exist a single significant development in philosophy¹ based on them; in fact, some uses of these notions seem quite contentless such as the familiar 'explanation' of the consistency of our mathematical results by saying that our results agree because we deal with the same objects. As I see it, the position is similar to that of the notions of the atom or absolute simultaneity at the time of the Greeks: neither of them could be used for understanding the world at the stage of technical and conceptual development of that time. (I chose the atom as an example of a notion with a future, and absolute simultaneity as an example of a notion which was later discarded.) In other words, the notion of a mathematical object is defective because one has no clue for using it to provide satisfactory answers to the (philosophically significant) questions which it should answer, but no case has been made out that it cannot do so—rather like philosophy itself.

Now, granted that such 'metaphysical' notions as that of a mathematical object are to be avoided in favour of an empiricist approach in the sense of 1.2*d*, it seems quite natural that Wittgenstein should be concerned with the positions 1.2*a*, 1.2*b*, 1.2*c*. Partly their attraction is due to wishful

¹ The content of this notion does not seem to lie here at all, but in the conception of set which it suggests (cf. section 2.2). The inconsistency of the naïve notion of a set is no more a defect in the concept of a mathematical object, than the Greek conception of a star as a hole in the sky is a defect in our conception of a star as a hot gas. Incidentally, it should be noted that Wittgenstein argues against a notion of a mathematical object (presumably: substance), but, at least in places (p. 124, 35 or p. 96, 71, lines 5 and 4 from below) not against the objectivity of mathematics, especially through his recognition of formal facts (p. 128, 50).

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thinking, but also there are valid, albeit inconclusive, reasons in their favour. I begin with the former.

As to 1.2*a*, it is clear that language is something 'tangible', not a 'hidden' object; as to 1.2*b*, proofs are recognised spontaneously, like colour (cf. p. 96, 70 or p. 125, 36), in contrast to theorems which, when regarded as assertions about mathematical objects, are inaccessible; as to 1.2*c*, experiments are something practical and they tell us facts (p. 99, 76 or p. 171, 14). Moreover, from these points of view the problem of an ultimate justification for mathematical assertions does not arise: in case 1.2*a*, since there is no sense in speaking of the truth of a rule anyway; in 1.2*b*, since there is no sense in speaking of the truth of an assertion before its meaning has been fixed, and so, if the proof is needed to determine the meaning of a theorem, then there can be no question of a justification for principles of proof; and in 1.2*c*, one is simply seeing what is happening and this is supposed to be unproblematic.

Of course, these formulations are oversimplified, and the positions 1.2*a*–1.2*c* are not plausible just because it is obvious that there are real problems connected with the justification of principles of proof. But it is interesting to see directly where these positions go wrong. Wittgenstein's remarks on this will be briefly touched in 2.1–2.3. Also I shall try to indicate roughly what is sound about these positions.

Note that 1.2*a* and 1.2*b* are more or less complementary because 1.2*a* leaves open the exact rôle of proof in mathematics, while 1.2*b* is mainly concerned with this question. Position 1.2*c* is contradictory to the others. What is common to them is that, separately, they are in the direction of an empiricist approach.

2.1. In favour of 1.2*a* we can say that, whatever else a theorem may be, it certainly is also a rule of inference: e.g. if A is a theorem, $A \rightarrow B$ is taken to imply B . Regarded as a reduction, 1.2*a* is open to the objection of 1.3 above. Wittgenstein stresses (p. 184, 32) a more interesting point which shows a general limitation of crude empiricism: while he regards 'believing oneself to follow a rule' as an empirical notion, the notion of following a rule correctly is not. In short, there is a non-empirical residue in the notion of a rule of language. The same applies also to the formalist conception of mathematics as a manipulation of symbols because the mathematical content does not lie in the physical production of the symbols, but in the formal fact that the symbols are produced in accordance with the given rule, or, as Wittgenstein prefers to put it (p. 81, 38), in our accepting the sequence of symbols as an application of the rule. He goes on to say 'but once we go in for this business of acceptance, then it need not be of a geometrical nature', i.e. it need not be restricted to construction of sequences of symbols according to syntactic rules. The point seems to me of importance for foundations generally, but Wittgenstein does not develop it.

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Position 1.2a raises the question why proofs are needed since a rule of language, as ordinarily understood, is a matter of simple decision. Wittgenstein does not give a plain answer, but suggests (p. 77, 28) that the proof tells us how to use the rule.

2.2. A parallel development to this suggestion is provided by 1.2b, e.g. p. 144, 4: before the proof is given the concept is pliable. The doctrine that the proof determines the meaning of the theorem, is familiar from intuitionist writings. As Gödel has pointed out to me (but see also p. 93, 61) the doctrine is supported at the level of computations where one considers symbolic operations with numerals in contrast to assertions about numbers considered as characteristics of sets: ' $5 + 7 = 12$ ', at this level, means that this equation is the last of some sequence of equations obtained by the application of certain rules, and the proof goes just the one step further of exhibiting this sequence. But as soon as one regards numbers as characteristics of sets one can meaningfully ask whether certain computational rules are correct, and to this extent statements about numbers have meaning independent of the rules of proof considered. Quite generally, it is simply not true that proof is primary and theorem derived, that only the proof determines the content of a theorem. In fact, Wittgenstein is wrong in saying that generally we change our way of looking at a theorem during the proof (p. 122, 30), but equally often we change our way of looking at the proof as a result of restating the theorem; e.g. if we are accustomed to the principle of proof that the totality of all subsets of a set is itself a set, we may reject it when it is pointed out to us that it is only valid for the notion of a combinatorial set and not, e.g. for the notion of a set as a rule of construction. (I chose this example, instead, e.g. cancellation by zero, because in the present case a restatement of the theorem is involved.)

2.2a. I believe that Wittgenstein's violent dislike of the consistency problem is connected with the thesis that *proof* is the fundamental concept. For, on the one hand it is difficult to understand how it is that different correct proofs do not lead to contradictory results, and that the calculations of different people agree. On the other hand there seems to be no approach to this problem on an empiricist basis. Wittgenstein proceeds as follows. He minimises the importance of consistency (p. 105, 81) by saying that he could imagine people who would be proud of a contradiction: but why should one attach more weight to Wittgenstein's imaginings than to the fact that people are not? He attributes (p. 122, 30) the agreement between different people to a similarity of training, though presumably the same could be said about the agreement between reports of the same physical event by different people; or (p. 13, 35) simply calls the agreement interesting, obviously implying that one should recognise this as a necessary condition for mathematics (p. 164) and not ask for explanations.

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The situation is quite unsatisfactory. We shall return to a more detailed discussion of consistency in section II.

2.2b. Gödel's remark in section 2.2 is a particular case of a general approach to the relation between *proof* and *theorem*, which has been developed by him. (The discussion below is a slight elaboration.)

The proof is not said to *determine* the meaning, but to *enter into* the meaning of a theorem as follows: the assertion A is interpreted as: A has (actually) been proved, say by a particular individual. On this interpretation, trivially A is false before a proof is given but of course not necessarily absurd. The fact, that A is false, is not of mathematical interest, that A is true is, but from this we can only conclude that therefore it always will be true, not that it always was. Clearly this is not the common interpretation of mathematical assertions.

What has been said so far is of course clear. But significant results are obtained if the interpretation is carried through thoroughly, for then *a theorem becomes an assertion about the actual structure of its own proof*. For this one interprets $B \rightarrow C$ as: it has been proved that if B has been proved then so has C , and similarly with the other logical connectives. (Absurdity is used only if we have a restriction on proofs which excludes proofs of a formula A , so that for each A we may assert $A \rightarrow A$ on the present interpretation.) In propositional reasoning the following stipulation about the individual whose proofs are considered must be made: at each stage he keeps his set of theorems closed under *modus ponens*. Note that the interpretation of the symbol ' $\rightarrow$ ' is not circular, because the '*if . . . then*' in '*if B has been proved then so has C* ' applies to propositions about a concretely specified totality, namely the proofs actually constructed by the individual at a given time, and so the truth functional interpretation is quite unproblematic here. Note also that only a minimum of conditions is imposed on the proof procedures employed by the individual considered.

For more sophisticated kinds of reasoning, where as, e.g. in number theory substitution of arbitrary numerals is permitted, it is unlikely that the necessary closure conditions on the set of theorems can actually be realised, because the set would be infinite. However it seems plausible that the interpretation applies if ' *B has been proved*' is replaced by ' *B can be proved from the theorems already constructed by means of (suitably chosen) decidable rules*' so that the logical connectives in the interpretation remain unproblematic. An exact study of this would seem to be desirable.

To repeat: we have here an interpretation of (a certain class of) mathematical assertions according to which the latter are 'about' empirically given sets of proofs satisfying certain closure conditions. It is not the only, or even most natural interpretation. No doubt the phrase 'the concept is pliable before the proof is given' is elastic enough; but it is difficult to say whether it will stretch to cover the present section.

2.3. Wittgenstein does not support position 1.2c at all (p. 124, 35 or p. 173, line 5 from bottom). His criticism of 1.2c leads to a general criticism of naïve empiricism: one needs concepts to tell us what are facts and mathematics provides them and handles them. Because of the general implications of this observation I do not feel competent to discuss it in detail, nor do I know how closely it is connected with the familiar claim that concepts are needed in the description of nature. But Wittgenstein's presentation of this view seems specially vivid, particularly in his discussion of the difference between measurement¹ and experiment (p. 98, 74): the same physical acts constitute in one case a measurement, in the other an experiment.

2.3a. The fact just mentioned immediately raises the question wherein this difference lies. It would be wholly barren to reply that it lies in the intention of the agent and that it is to be discovered by asking him whether he is making an experiment or a measurement. Wittgenstein wishes to remove the distinction from the sphere of psychology by saying that whether an act is a measurement or an experiment depends on the language game of which it is a part. It is not at all clear to me that this nice word 'language game' really clarifies the traditional term 'meaning', and does not merely replace it.

There seems to be something psychological in the distinction considered, because, with our present experience of machines, it does not seem possible to say significantly of a machine that it is making a measurement or an experiment: instead, we use it for such a purpose (cf. p. 133, 2: does a calculating machine calculate?).

Wittgenstein employs several other notions with a similar psychological flavour in his attempts to characterise (certain aspects of) proofs, e.g. p. 14, 39: to impress a procedure upon someone; p. 18, 53: to use something as a picture; p. 72, 14: to use it as a paradigm; p. 45, 153: to take in a structure at a glance, etc.

2.4. This completes a very rough review of Wittgenstein's remarks on general philosophy. My impression is that he has not put forward a coherent framework within which one can significantly discuss the nature of mathematics. We are left with a shambles, although the remarks

¹ He also talks about the difference between calculation and experiment (p. 95, 69). The discussion should be elucidated; first, it would be more reasonable to confine attention to the difference between calculating and experimenting in *mathematics* since otherwise the most obvious differences of subject matter obscure the differences at issue; second, Wittgenstein also compares calculating with measuring and with setting up means of measurement. The latter is at least as problematical as calculating: consider, e.g. the theoretical problems involved in setting up a unit of time which, contrary to popular misconceptions, is not done by setting up some conventional piece of apparatus and *defining* it to be the measure of time.

contain some sound criticisms of the naïve empiricist approach in the sense of 1.2*d*.

The remainder of the review, except for a brief digression in section 3.3, is concerned with the foundations of mathematics in the sense of section 1.

3 Wittgenstein's General Conclusions

I consider the conclusions which I have attributed to Wittgenstein in 1.3*b*, confining myself mainly to points of specifically mathematical interest. I do not accept his conclusions since I do not think that they are fruitful for further research. The points raised in the present section and the discussion of 8.4 below seem to me to show that *the value of the book does not lie in a new point of view, but in penetrating observations and questions on a limited subject matter.*

3.1. Even though the aims of the traditional schools of philosophy in their crude form are unattainable, I do not see why these aims should not be modified in the light of criticism, and then pursued. Thus, though the requirements of strict empiricism cannot be satisfied, a leaning towards it seems fruitful. It seems to me that Lorenzen's operative logic constitutes an interesting formulation of the idea that theorems are rules of language: Lorenzen, of course, does not try to reduce the act of following a rule (correctly) to empiricist terms, but starts with the fact (or: idealisation) that we do follow rules. And notwithstanding the criticisms of 2.2*a* and 2.2*b*, we can develop a mathematics based on the primitive concept of *proof* or even *constructive proof*, as the intuitionists have done (6): we ignore the problem of how we come to recognise a proof and start from the fact (or: idealisation) that we do. Only, as stated in 2.1, it is not likely that one approach will turn out to be 'more unique' than the other (cf. also 8.3*b*).

3.2. As far as mathematical foundations are concerned, it is certainly true that if mathematical concepts are used in foundations, they are liable to raise the same type of problem as they are supposed to answer: we are left with elucidations rather than explanations (see Gödel on consistency proofs, impredicativity of the notion of intuitionistic proof, and of a set: to mention difficulties met by the three most prominent mathematical approaches to the foundations of mathematics). But to quote out of context (p. 174, 16): don't demand too much and have no fear of your problems dissolving into nothing. In fact, mathematical logic has done far more to get an overall view of mathematics, to help us find our way about (p. 104, 80), than any other single discipline: it provided concepts necessary for the description of mathematics, just as, according to Wittgenstein, mathematics provides the concepts necessary in the description of nature.

Wittgenstein's views on mathematical logic are not worth much because he knew very little and what he knew was confined to the Frege-Russell

line of goods.¹ But it is true that the methods of mathematical logic have not been applied successfully to the subject of elementary computations (7.1), and this is precisely the subject which interested him most.

3.3. As to Wittgenstein's conception of philosophy, there are two questions: the rejection of other conceptions and the promise of his own, the clarification of the grammar of mathematics. He does not say exactly wherein this consists, but it is evidently to do with applications and intellectual institutions (e.g. p. 176, 18 last paragraph, or p. 173, paragraph 8). The concepts used in his description of our mathematical activity are those of 2.34. Now, I see no objection to them on formal grounds, and I can believe that they afford a better framework than either a purely empiricist (behaviourist) or a purely introspective analysis. But these notions and the whole programme of clarifying the grammar remind me of the 'soft ² options' at school: human geography (not the mineral composition of mountains, but their effect on history) or economic chemistry (not the atomic structure of the chemical elements, but their uses in society). It is all sensible and interesting, not hackneyed (p. 170), but often bypasses the problems which later turn out to be most fruitful. But even if this conception turns out to be useful, there is no clear reason for rejecting the others, no more than that human geography should exclude scientific geology. There are such traditional problems as the genesis of our mathematical concepts, the justification of proofs, i.e. what makes them correct rather than what makes them interesting, which are the fundamental concepts and which derived: we need a conceptual apparatus to formulate these questions in a satisfactory way, and we do not have such an apparatus. But it seems unlikely that the concepts favoured by Wittgenstein will provide it. It is by no means clear that these questions are ripe for a precise formulation, any more than the general (and natural) questions of present day mathematics were ripe for a formulation at the time of the Greeks.

4

There is another, less austere conception of the philosophy of mathematics, which Wittgenstein ignores too. Since this conception, it seems to me, underlies most of current work in mathematical logic, he implicitly rejects it by rejecting mathematical logic. As mathematics has grown, a variety of different methods of proof, definitions, theorems have

¹ This is not at all typical of the subject. However, I am told that many professional philosophers are similarly uneducated and are, therefore, likely to have similarly distorted views.

² This impression of philosophy is supported by Wittgenstein's general comments on his conception of philosophy: perhaps one should not pay too much attention to them since some passages in the book (cf. Sections 7 and 8 below) involve 'hard analysis'.

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accumulated. By the light of nature we see differences, groupings within one branch, and similarities between different branches of mathematics. One may see one aim of a philosophy of mathematics in getting a clear understanding of these connections, and there is no reason in advance why this should be done only by reference to 'applications', and not, e.g. by mathematical properties, by mathematical characterisations. From this point of view it is a contribution to the philosophy of mathematics if a new aspect of the methods of mathematics has been noticed, such as, e.g. Wittgenstein's own observations discussed in sections 7 and 8 below; here there is no one fundamental problem. I regard the 'rival' philosophies of mathematics in this light: not as contradictory in substance, but as emphasising different aspects of mathematics (cf. writings of Bernays quoted below).

4.1. It should be observed that the originators of the rival philosophies took a different view: they insisted on rejecting those aspects of mathematics which they did not consider themselves; like a woman who fears one would not be interested in her if one remembered that others existed besides her. Of course, from the point of view of a *Lebensphilosophie* these rival philosophies are contradictory because they regard different aspects of mathematics as specially important.

5 Background (Set Theory)

The historical background to Wittgenstein's remarks consists of two parts. First there is the logicistic reduction of mathematics to logic and abstract set theory: many of Wittgenstein's remarks are a reaction against this. Second, there are the so-called constructive tendencies which are also a reaction to the logicistic approach or, at least, are in a different direction. We describe the reduction to set theory first, and then some brands of constructivism in order to compare Wittgenstein's views with them.

Abstract set theory provides the most famous of all foundations of mathematics. The remarkable fact is this: each known branch of mathematics has a model in abstract set theory, and frequently, a most natural model. Thus, e.g. the question 'what is a number' to which it is hard to give a natural meaning, gets the answer: an element of the set which is the intersection of all inductive sets. Similarly, the corresponding questions for other mathematical concepts are answered in this uniform way. It is fair to say that this programme of regarding our mathematics from this point of view of set theory has been carried out in far more detail than, e.g. the programme of describing the physical world as assemblages of fundamental particles. (The common feature of these programmes is that they reduce the *number* of 'primitives'.) In particular, the development of arithmetic within set theory has not only helped us to understand

arithmetic better, but has had important repercussions on the study of set theory and logic generally, e.g. it permits the application of Gödel's incompleteness theorem to systems of set theory, and related results.

5.1. The interest of the discovery just described cannot be doubted. But it is not so clear that it satisfies the philosopher who seeks a 'simpler' foundation, who looks for the fundamental concepts in mathematics and wants to build up derived ones. Those philosophers who see the content of a scientific statement in the observational verification will deny that the fundamental particles are simpler than the physical objects of our experience, but they cannot deny that at least they are smaller. There is no obvious order in which abstract sets precede numbers, as the fundamental particles precede objects of our immediate experience in size. And, as e.g. Poincaré pointed out with great lucidity, the reduction of arithmetic to set theory itself requires the processes of arithmetic. If the notion of number presupposes the concept of a finite set, even then, as Lorenzen has observed,¹ this does not mean that one has to consider arbitrary sets first and then restrict them to finite ones. In other words, in this search for foundations, for notions with a more elementary content, one may wish to select parts of mathematics, or, in particular, parts of set theory. Wittgenstein emphasises strongly the mathematical significance of such selections.

5.2. Another respect in which the set theoretic foundations fail, is in characterising the constructive aspects of mathematics which Bernays calls, epigrammatically, a mathematics of doing (cf. also p. 118, 15) in contrast to a mathematics of being, or, more formally, an idealisation of process as against an idealisation of what is the case. Neither of these conceptions can be expected to be fully comprehended by the other; although there are interesting formal relations between them. It is interesting to note that there is not only a constructivist critique of so-called classical ('platonistic') mathematics, but also a converse. In fact, many mathematicians are almost proud to declare that they don't understand intuitionism (or the other constructive tendencies); they ask: how do you define constructive proof? and do not really expect an answer; rightly as long as they presuppose a definition in set theoretical terms. Their complaint about the vagueness (meaninglessness) of the notion of constructive proof is on a par with the intuitionist complaint about the meaninglessness of the notion of arbitrary functions: how do you specify them, since they are supposed to be non-enumerable? Yet without giving a list of either arbitrary sets or of all constructive proofs, Zermelo laid down properties of the former notion in his set theory and Heyting of the latter in his axiom systems for intuitionistic logic. From now on we shall be mainly concerned with the constructive side and we discuss it on its merits without attempting to reduce it to set theoretical terms.

¹ *Math. Ann.*, 1951, 123, 331-338

6 Background (Constructivity)

It is the custom to lump together all constructive tendencies under the heading of *intuitionism*. To get a little more orientation we shall distinguish between intuitionism proper as developed by Brouwer and Heyting, and *finitism*. There is an even narrower conception of constructive mathematics, namely *strict finitism*, a notion described by Bernays in 'Sur le platonisme dans les mathématiques'.¹ Wittgenstein's views seem related and favourable to intuitionism, probably mainly because of common features such as the objection to the idea of a mathematical object, the priority attached to proofs over theorems (cf. 2.2), and the use of Brouwer's household example of the decimal expansion of π (p. 138, 9 or p. 144, 19, or p. 147, 27). But a closer look shows that this similarity is superficial, and that Wittgenstein's views on mathematics are near those of strict finitism; or, perhaps one should say, he concentrates on the strictly finitist aspects of mathematics. To justify this assertion it is necessary to describe some differences between these three constructivist tendencies. (Strict finitism will be considered in the next section.)

6.1. First, we describe differences on a general (epistemological) level. Hilbert-Bernays² states that intuitionism permits the use of general logical considerations in addition to the combinatorial facts with which finitist mathematics is concerned. Intuitionism deals with '*mental constructions*' while a finitist piece of mathematics is to be regarded as a Gedanken experiment (description of an experiment) with *concrete* objects³ which are thought of as reproducible, are to be recognisable, and surveyable, i.e. thought of as built up of discrete parts whose structure can be surveyed.⁴ Intuitionism includes finitism because a picture of a concrete object can be used in a mental construction, as this word is employed by the intuitionists. But it goes beyond finitism because it makes statements concerning all possible constructions, which certainly do not constitute a concrete totality. These, and similar, general differences become apparent in the typical features of the notion of intuitionist as opposed to finitist proof: a false proposition implies anything; undecided propositions, and even implications between such propositions, may be used as premisses in implications, i.e. one makes assertions which involve an hypothetical proof, namely a proof of the premise, though the totality of all proofs is not concretely specified (it is regarded as an accident that in most proofs of implications $A \rightarrow B$ the details of the proof of A are not used in the proof of B , as, e.g. in $[A \ \& \ (A \rightarrow B)] \rightarrow B$ where one need merely attach the proof of $A \rightarrow B$ to the

¹ *Enseignement mathématique*, 1935, 34, 52-69

² *Grundlagen der Mathematik*, 1934 I, 43

³ Ibid, p. 20, line 8 from foot.

⁴ Ibid, p. 21, lines 21-23, where Bernays uses '*überblickbar*' instead of Wittgenstein's '*überschbar*', e.g. p. 65, I, with its ambiguity of 'overlook' and 'look over'.

proof of A in order to get a proof of B, though, in fact, Brouwer's proof of the fan theorem is the only known example of the contrary case); as a result it is not clear what 'construction' constitutes the content of such an implication: this applies in particular to the double negations of which intuitionist writings are full.

6.1a. Granted that features of this kind are typical of the difference between finitist and intuitionist mathematics, it is clear that it is unprofitable to compare Wittgenstein's views and intuitionism. For, in his simple computational examples we are dealing with strictly combinatorial processes, and the typically intuitionist concepts do not apply: he leaves off before intuitionism starts.

6.2. Finitist mathematics does not use the general notion of a constructive proof at all, in fact it might be said to avoid *logical* inferences (which involve an impredicative concept of proof) because it is restricted to purely combinatorial operations. In particular, the logical connectives have a purely combinatorial character since they are applied only to decidable formulae and so the truth functional interpretation (truth table method) is not problematic. Universal quantifiers are not used at all except in so far as they can be replaced by free variables, e.g. not in the premiss of an implication. Existential quantifiers are used as shorthand for a (constructive) function or functional: if they occur in a premiss, e.g. $(\text{Ex}) A(x) \rightarrow B$, they are interpreted as $A(n) \rightarrow B$, when n is a free variable which does not occur in $A(x)$ nor in B . Iterated implications are not used at all. These restrictions all follow more or less cogently from the general conception of a mathematics about concretely presented objects.

6.2a. The reason why there is such general confusion about the difference between finitist and intuitionist proofs is simply this: one very rarely uses all that intuitionism would allow, e.g. up to the present we do not know, for A known to be recursive, a single intuitionist proof of $\sim(x) A(x)$ where we cannot specify an n such that $\sim A(n)$ (though the general conception of an intuitionist proof makes it plausible that there are such A); again $(x) A(x) \rightarrow (y) B(y)$ can always be sharpened in known cases to $A[f(n)] \rightarrow B(n)$ where $f(n)$ is a specified (constructive) function. Iterated implications are extremely rare in mathematics anyway.

6.3. In short, all the mathematics which Wittgenstein considers clear (not, e.g. the completeness of the set of real numbers) fits comfortably within the framework of finitist mathematics, and so, as stated in 6.1a, it is futile to compare his views with intuitionism. In fact it will turn out that an even narrower aspect of mathematics is considered by him.

7 Strict finitism

Finitism is, of course, an idealisation too, i.e. it ignores certain differences and distinctions based upon them. In particular, it does not distinguish

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between constructions which consist of a finite number of steps and those which can actually be carried out, or between configurations which consist of a finite number of discrete parts and those which can actually be kept in mind (or surveyed). In fact within any given degree of sophistication of proofs a classification according to the degree of complexity is most natural. Wittgenstein stresses (p. 65, 2) the further point that explicit definitions and new notations may convert a piece of mathematics which is not strictly finitist into one.

7.1. No rigorous work has been done on this subject, partly, no doubt, because the current methods of mathematical logic do not seem to lend themselves to it. Perhaps the study of finite computing machines or the human learning process will make the problems which arise here practically pressing. But also, so far, this concept of finitist proof in the strict sense has not been applied to questions which interest logicians such as the general notion of 'equivalence of proofs' or 'content of proofs'. It seems to me that Wittgenstein gives some very interesting hints in this connection which will be described in the next section.

7.2. To avoid misunderstanding: I myself have not worked on strict finitism, and do not know what direction research on it might take. But Bernays and Wittgenstein have certainly drawn attention to a new *area* of research in foundations. Those logicians and philosophers whose taste leads them to work in developed fields where others have created concepts on which they can model their own, will certainly not be attracted to strict finitism at the present stage; just as they would not have been attracted to intuitionism before Heyting analysed its formal structure and Tarski important aspects of its interpretation. Hao Wang continues this discussion in a forthcoming article in *Dialectica*; in particular Wittgenstein's concern (para. 2.2) with the relation between proof and theorem is seen in a new light if one broadens the notion of strict finitism to *that which one can actually grasp*; for, after one has gone through a proof one actually handles a theorem differently from before. The suggestion is undoubtedly in the right direction.

8 *Equivalence of Proofs*

I shall discuss Wittgenstein's views by reference to his criticism of the reduction of arithmetic to logic and to some remarks of his on non-constructive existence proofs in analysis. It seems to me that what he has to say about the reduction of arithmetic to logic is not concerned with any peculiarities of the branches of mathematics considered, but applies generally to *proof theoretical reductions* or *translations*, either mapping of proofs of one kind into proofs of another, or theorems (provable formulae) of one system into theorems of another.

8.1. Wittgenstein's first point concerning the reduction of numerical arithmetic to logic with identity is this: we do not really have a reduction

here because, by the methods of logic alone, we could not decide whether a particular formula of logic corresponds to some given formula of numerical arithmetic. Also there would always be the question of whether a rule has been correctly applied (p. 89, 53, or p. 91, 56).

8.1a. This point seems wholly acceptable, and, what is more, quite familiar: it concerns the metamathematical methods used for investigating relations between two systems. We do not speak of a 'reduction' unless the metamathematical methods are weaker in some suitable sense or, at least, more evident than the methods studied. For instance, in his reduction of formalised classical arithmetic to intuitionistic arithmetic, Gödel was careful to use finitist methods of proof. Again, if a system is decidable, i.e. if there is an effective method of associating with every formula of the system one of the formulae $0 = 0$, $0 = 1$ ($0 = 0$ with provable ones, $0 = 1$ with the others), we do not generally speak of a 'reduction' of the system to arithmetic modulo 2: for, in general, the method used for constructing this translation goes far beyond arithmetic modulo 2. In the colourless language of professional logicians one replies to Wittgenstein's question how we know that the rule (of translation) has been correctly applied by saying: the whole reduction must be considered relative to the methods of proof used in the metamathematical argument. And one would agree with him that the metamathematics required for a translation of arithmetic into logic requires some arithmetical concepts. However one would remember that without being a reduction such a translation can be of central importance (cf. end of section 5).

8.1b. Wittgenstein's next point is much more positive. Instead of just saying that the metamathematical methods used in the translation include the methods studied, he looks for appropriate weaker methods, and observes, e.g. that the reduction of the decimal notation for numerals to the stroke notation (p. 66, 3) cannot be done by strictly finitist methods. He thereby uses the notion of strict finitism for making a natural distinction, which is not made customarily.

8.2. Wittgenstein repeatedly raises the question of characterising the equivalence of proofs in contrast to equivalence of results (p. 69, 8; p. 69, 9; p. 66, 3 last paragraph). This is a somewhat elusive notion, a little like Heyting's notion of the completeness of a calculus with respect to proofs and not only with respect to results,¹ but in particular cases it is clear enough. For instance, Shoenfield² showed how to replace the induction schema in the elementary quantifier-free arithmetic of addition by means of a finite number of axioms, including $a + b = b + a$, without altering the set of theorems. Now, it is intuitively clear that the proof of $a + b = b + a$

¹ Cf. his book *Intuitionism. An Introduction*, p. 102.

² *Journal of Symbolic Logic*, 1957, 22, 112

by induction does not have an 'equivalent' counterpart in the finitely axiomatised system.

8.2a. Wittgenstein does not succeed in characterising this notion of equivalence or in comparing¹ proofs. He recognises (p. 93, 60) that under too strict a criterion of equivalence a proof will be equivalent only to itself: he does not see any objection to this, and does not attempt to find fruitful criteria. But, in my opinion, he raises an interesting problem here.

8.3. He does attempt to find a characterisation of a very general sort by basing a comparison of proofs on the application,² or, as he puts it (p. 155, 46), on what I can do with it. I believe that at a certain level this is a useful approach, but its limitations are more interesting.

8.3a. Wittgenstein makes the remark in connection with non-constructive existence proofs in which I myself have been specially interested. Suppose we have a proof of $(\exists x)A(x)$ or $(\exists x)(y)B(x, y)$ where we think of the variables as ranging over natural numbers. The first thing that I may expect to 'do' with the proof of such a formula is to read off from it instructions for calculating an n such that $A(n)$ holds. (With the ordinary methods of proof this can be done if A is recursive.) In general I cannot 'do' this in the second case, e.g. if we have a proof (naturally by *reductio ad absurdum*) of $\sim(x)(\exists y) \sim B(x, y)$. Such a proof would generally proceed as follows: suppose $(x)(\exists y) \sim B(x, y)$, then for a certain function $Y(x)$, $(x) \sim B[x, Y(x)]$; the proof (in general) shows $\sim(x) \sim B[x, Y(x)]$ by constructing explicitly an x_y (depending on Y) such that

$$B[x_y, Y(x_y)]. \quad (1)$$

This formula, with its construction of (the functional) x_y in terms of Y , may be said to tell us what we can 'do' with the non-constructive existence proof.³

I do not think that this analysis is at all artificial: but it certainly presupposes that we are looking for something of this sort, i.e. that we wish to express 'what we can do with the non-constructive proof' in terms of some quantifier-free (possibly finitist) assertion like (1) above. Perhaps

¹ An incidental observation: I have sometimes felt that Wittgenstein's violent dislike of the notion of mathematical truth is connected with comparisons of proofs. He could see that the comparison of proofs was informative, and he didn't have a snappy answer to the objection sometimes made by mathematicians: But all we want to know is whether the theorem is *true*. The answer is: If you think about what you are saying you will see that you are wrong.

² There seems to be a conflict with p. 157, 52, where he says that it is useless in the philosophy of mathematics to reformulate proofs: one would have thought that this might exhibit the application especially clearly.

³ All this involves a restatement of the theorem and a consequent reformulation of the proof without, as one says, changing the idea of the proof: cf. the preceding footnote.

it is particularly easy to say what we can 'do' with a non-constructive existence proof because the matter of constructivity is at issue, and so a complete elimination of the non-constructive use of the quantifier is all we want, and we get it. The situation appears even simpler *post hoc* because also from a 'non-constructive' classical point of view, (1) is better than $(\text{Ex})(y) B(x, y)$ in the following sense: $(\text{Ex})(y) B(x, y)$ follows from $B[x_y, Y(x_y)]$ with the variable Y by pure (classical) logic, but not the converse: graphically, (1) is more informative than $(\text{Ex})(y) B(x, y)$.

8.3*b*. But it seems to me that here we have a very special case in the foundations of mathematics, because the requirements on a satisfactory solution are pretty clear: as we have said above, the adjective 'non-constructive' itself suggests what we should aim for. And in any case, what we can 'do' with the non-constructive proof is more informative than the non-constructive theorem.

But in the more fundamental problems of the foundations of mathematics, the question 'what can I do with it' does not seem to help. Suppose I have a finitist and a non-finitist proof of a universal formula $(x)A(x)$: What can I 'do' with the former which I cannot do with the latter? In both cases, for each numeral n (errors excepted) we shall have $A(n)$. Incidentally, comparison between the intuitionist proof of the fan theorem and König's proof of the corresponding theorem in classical mathematics raises the same question:¹ what can I 'do' with one, what with the other? Nothing like the question of 'estimates' for existential quantifiers, which is central in the cases of 8.3*a*, is relevant here. What one is left with is simply this: if one starts with the classical conception Brouwer's proof is simply not valid because he makes an unjustified restriction on the methods of proof of the hypothesis which in any case is not relevant because one only wishes to assume its truth, and if one starts with the intuitionistic conception König's proof is not valid because he employs a (provably) non-constructive least number operator. I do not see any 'practical purpose' or considerations of 'usefulness' which could decide between the two proofs.

8.4. It is my impression that the emphasis on 'application' or 'on what we can do with it' aims at unifying our point of view: we are to look at two conceptions like the classical and intuitionist conception of mathematics, and find a place for each from our point of view, namely: according to their applications. Mathematicians sometimes pretend to a similar criterion of judgment, namely: 'mathematical fruitfulness'. Everybody knows that in subtle cases the criterion of fruitfulness usually does not apply because people differ in just these cases in what they find

¹ The rest of para. 8. 3*b* presupposes acquaintance with the proofs of Brouwer's fan theorem, e.g. in Heyting's book cited in section 8.2 above, and of the Unendlichekettenslemma.

interesting. From 8.3*b* it appears that in subtle cases Wittgenstein's notion of application is no better. The aim is not achieved, and this fact seems to support the conclusion of section 3 above.

9 Higher Mathematics

The remainder of this review deals with isolated topics which are discussed at length in the book. I make no attempt to relate them to a general point of view. Some of them are so absurd that they seem due to very general misconceptions and not just carelessness (cf. p. 55, lines 11 and 12). It might be interesting to trace such connections.

Wittgenstein says (p. 58, 6) that it was the diagonal argument which gave sense to the assertion that the set of all sequences (of natural numbers) is not enumerable. The definition is: a set of sequences $a(1), \dots, a(m), \dots$ is enumerable if there is a double sequence $s(n, 1), \dots, s(n, m), \dots$, $n = 1, 2, \dots$ with the following property: for each sequence of the set, there is an n_a such that $a(1), \dots$ is identical with $s(n_a, 1), \dots$, i.e. for all m , $a(m) = s(n_a, m)$. And the diagonal argument states, that the set of all sequences is not enumerable because (i) if $s(n, m)$ is a double sequence, then $s(n, n) + 1$ is also a sequence and so (ii) any proposed enumeration $s(n, m)$ fails to include one sequence, namely $s(n, n) + 1$. One could only wish that all one's assertions had as much sense as the assertion of the non-enumerability of the set of all sequences before its proof!

9.1. What is wrong here? Well, after all there was a paradox, Skolem's paradox, which puzzled people. The mistake is to think that the diagonal argument applies *only* to the set of all sequences: given any set of simple and double sequences satisfying (i), the non-enumerability of these simple sequences by a double sequence of the set is established. In other words we may expect quite 'small' models of a set theory which satisfy the nonenumerability condition, since not only the sequences but also the means of enumeration of these sequences are restricted in such a model. But this does not at all mean that the set of all sequences is ill defined: the fact remains this set has the property (i).

10

Wittgenstein criticises Gödel's first incompleteness theorem; or, at least, the part which states that if a suitable system of arithmetic is consistent then there is a true formula of the form $(n)A(n)$ which is not provable in the system; the formula is one with number q which states: for every n , n is not the number of a proof of the formula with number q , i.e. a proof of itself. The arguments are wild, including such points as an inconsistency wouldn't matter (p. 51, 11), or how do we know that this is the correct translation of the arithmetic formula $(n)A(n)$ (p. 51, 10), or what does it mean to suppose that a formula is provable (p. 177, bottom). Even if

an inconsistency didn't 'matter', one cannot hope to discuss significantly on this basis a result which explicitly supposes consistency of the system. Why does he think that the elaborate details of Gödel's paper are needed? Just because Gödel has to show that on the assumption of consistency the proposed translation is correct. And finally, one of the major purposes of considering formal systems (and it is formal systems which Gödel considers) is that a clear combinatorial (geometric) meaning is given to a formula being provable.

10.1. Just as with the diagonal argument, nothing is *wrong* here, but perhaps the following explanations are appropriate.

First of all, logicians are quite aware of the choices open in translating a syntactic assertion in English into arithmetic form; in fact, Rosser's variant of Gödel's proof depends just on this fact. For the first undecidability theorem all that is needed of a translation of the proof predicate (m is — the number of — a proof of — the formula with number — n) is this: there should be an expression $\mathcal{P}(x, y)$ in the system such that $\mathcal{P}(o^{(m)}, o^{(n)})$ is provable (refutable) in the system for the numerals $o^{(m)}$ and $o^{(n)}$ just in case m is (not) a proof of n . And this is ensured by consistency and the other conditions imposed on the system. For a significant formulation of the second undecidability theorem the expression $\mathcal{P}$ has to be so chosen that one can 'see' that it possesses certain properties of the proof predicate; i.e. one must be able to prove formally in the system certain implications involving $\mathcal{P}$. This was first analysed by Hilbert-Bernays and recently Feferman has obtained interesting further results. This work shows what a workmanlike approach to such a matter as an ambiguity in translation should be: it is not regarded as an inherent defect, but it is analysed till one sees what is actually needed in a particular context.

The beauty of Gödel's own formulation is that his result can be separated from the question of truth in arithmetic. He has found a formula $(x)A(x)$, A primitive recursive, which is formally undecidable in the given system $\mathcal{S}$ if $\mathcal{S}$ is consistent. Now, given this syntactic result one argues: since any closed proposition is true or false, either $(x)A(x)$ or $\sim(x)A(x)$ is true, and so there is a true proposition which cannot be proved in $\mathcal{S}$ (on the intended interpretation of $\mathcal{S}$). One sees that all one needs of the concept of truth is that either $\mathcal{A}$ or $\sim\mathcal{A}$. Actually, our notion of truth is clear enough to argue: $(x)A(x)$ is true. For if it were false for the numeral $o^{(n)}$, then $\sim A(o^{(n)})$ would be provable in $\mathcal{S}$ (provided all recursive predicates can be computed in $\mathcal{S}$) and so $\sim(x)A(x)$ would be provable.

To me Gödel's results do not at all suggest that our intuitive notion of an integer is defective: I can't believe that anyone ever had an intuition that there was a recursive decision method for arithmetic, even if, like Hilbert, he wanted one. It is not a death blow to the axiomatic approach since we now consider partial instead of complete axiomatisations. Nor

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necessarily to Hilbert's consistency problem: one has to examine the methods employed in the consistency proof on their merits. In fact, what Gödel shows is that the methods employed in a consistency proof of a system of arithmetic cannot be more evident than the methods formalised in the system, simply by virtue of being more restrictive. However, just because of this Gödel's results did destroy Hilbert's aim of getting rid of all problems of foundations once and for all. To me, the most striking fact is this: while nobody has ever been troubled because the particular sentence of arithmetic is undecidable, so to speak because the systems are *open*, Gödel's own principle for deciding such sentences shows that they are *artificially* open: his principle is: if $\mathcal{A}$ is provable in the system then $\mathcal{A}$. This principle can be formulated in each of the usual systems of arithmetic, and yet it extends the system considered.

II

Wittgenstein's criticism of the consistency problem ranges from a proposal to use the double negation as an enforced negation (p. 53, 18) (when $\sim A$ and $\sim \sim A$ would not be contradictory), to the proposal of not drawing conclusions from a contradiction (p. 169, 12) (as described, e.g. in Ackermann's system of strict implication,¹ where the consistency problem is not relevant), to modifications in our arguments after we reach a contradiction (which is done every day, both as a correction of errors, oversights in definitions, etc.) In fact the consistency problem as conceived by Hilbert had a perfectly specific point: if one can prove, by limited ('understood') methods, the consistency of a system S , then if a universal recursive formula can be proved in S , then the same formula can be proved by the limited methods. Since he considered more elaborate formulae as 'ideal' elements, with no hope of assigning a clear constructive meaning to them,² this was the most he could expect from foundations of arithmetic. In addition, it turned out that consistency proofs gave a great deal of additional information.

II.1. Here, for once, Wittgenstein also makes a justified objection (p. 130, 56) among all the wild shots which miss the mark: the one-sidedness of the consistency problem. From the consistency we can draw some conclusions (sections 10, 11), perhaps more than one would expect. Yet, as Gödel has shown there are formally consistent systems of arithmetic, which are ω -inconsistent, i.e. $(\exists x)A(x)$ can be proved but for each numeral $0, 1, \dots$, $A(0), A(1), \dots$ can be refuted. This had obviously not been suspected. And even some of the results of the Hilbert school are more

¹ *Journal of Symbolic Logic*, 1956, 21, 113-128.

² Though this has been done by Herbrand and others (cf. Section 13).

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informatively described, e.g. as extended ϵ -theorems than as mere consistency results.

11.2. It is true that some popular reasons for the consistency problem are unacceptable. If, e.g. one's aim is to save 'our mathematics', but if, like Hilbert, one regards only universal formulae as significant and others as auxiliary (ideal) one has not saved anything because these formulae can be proved by the methods employed in the consistency proof itself. If one is interested in scientific applications, consistency is certainly not enough, the system must be correct for the intended application, and in this case consistency will be a by-product. (Wittgenstein says, p. 104, 80, that consistency will be a by-product if one aims at a useful application: the quantum physicists wish it were.) And indeed an inconsistency is not shattering. For, if one thinks of mathematics as a body of rules and finds an inconsistency one modifies their statement: Cantor's proofs still stand although they can be embedded in Frege's inconsistent system. And if one thinks of mathematics as concerned with mathematical objects, the progress of science shows how our basic conceptions (cf. page 138, n.) may turn out to be inconsistent with experience, yet many observations made previously still stand, and only those whose interpretation is strongly tied to the particular conception, are discarded.

Finally, as is often stressed by the physicists, if mathematics is used as a handmaiden of the sciences, an inconsistency of the rules obtained by an unintended application, even if this had not been excluded explicitly in advance, may be quite acceptable; once, a mathematician derived a contradiction from the definition of a function introduced by a famous physicist, upon which the latter commented: Did I make a mess of my function? In any case there are ambiguities and uncertainties in the physical assumptions, so why not put up with similar faults in the mathematical manipulation? In fact, as Wittgenstein observed explicitly (p. 189) one could do physics without distinguishing between mathematical and physical facts at all. In other words, we have the interesting fact that something to which our conception of mathematics does not apply, can also be used as a handmaiden of the sciences. But this fact does not invalidate our conception of mathematics which is more exacting than that just described.

When all this is recognised, the mathematical problem of consistency still stands, and is fruitful: proofs of consistency and, more generally, of independence yield, perhaps, a better control over a calculus than anything else. (In one place, Wittgenstein seems to agree with this: p. 106, lines 8, 7 from below). Also, the separation between mathematical and physical facts, even if artificial in places, has its value in the manipulation of physical theories, particularly when a modification of the theory is required. Of course, when we speak of the consistency of rules, we make a mathematical assertion, and ignore certain epistemological problems, such as the one which

Wittgenstein stresses particularly: how do we know that the rules have been correctly applied.¹ I agree that this is also a problem. But it is completely barren within the context of consistency because it questions the very basis from which the consistency problem derives its significance. For, this epistemological problem arises with every application of a rule (and, indeed, an analogous problem arises with all communication) while the consistency problem arises with specific sets of rules.

12

Wittgenstein simply did not know what to say about the paradoxes. I don't either. But one thing is clear: the fruitful problem is not to 'get rid of them' but to get something out of them. Gödel got his general results on formal systems out of one set of paradoxes, Loeb got a beautiful result out of another.

I myself have found the following version of Russell's paradox illuminating, though it does not seem to lend itself to generalisation.

Recall first that in one definition of natural numbers in set theory, the null set corresponds to the number 0 and, if N corresponds to n , $N_0\{N\}$ corresponds to the successor of n . There is no greatest integer because $N_0\{N\}$ is not included in N .

Now let us consider any set A with the property ($\mathcal{P}$) that its members do not belong to themselves, i.e. $X \in A \rightarrow X \notin X$, and hence $A \notin A$. So $A_0\{A\}$ also has the property $\mathcal{P}$. In other words the Russell paradox involves the same argument as the theorem that there is no greatest integer, and at the same time suggests a natural way of generalising the successor construction. (From this point of view the Russell Paradox does not seem more astonishing than a child's assumption that there is a greatest integer: we have overlooked the fact that not every property has a definite extension.)

It is hard to say something really coherent about the paradoxes, but one can do better than speak of a head of Janus looking down on the other propositions (p. 131, 59), though I like the psychological accuracy of p. 131, 58.

13 *Personal Note*

I knew Wittgenstein from 1942 to his death. We spent a lot of time together talking about the foundations of mathematics, at a stage when I had read nothing on it other than the usual *Schundliteratur*. I realise now from this book that the topics raised were far from the centre of his interest though he never let me suspect it.

¹ What is right about his criticism is this: if one is concerned with reliability in a realistic sense, then it would be wrong to consider only the abstract rules and not their use.

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What remains to me of the agreeable illusions produced by the discussions of this period is, perhaps, this: every significant piece of mathematics has a solid mathematical core (p. 142, 16) and if we look honestly, we shall see it. That is why Hilbert-Bernays vol. II, and particularly Herbrand's theorem satisfied me: it separates out the combinatorial (quantifier-free) part of a proof (in predicate logic) which is specific to the particular case, from the 'logical' steps at the end. Certain interpretations of arithmetic and analysis have a similar appeal for me. I realise that there are other points of view,¹ but for the branches of mathematics just mentioned, I still see the mathematical core in the combinatorial or constructive aspect of the proof.

I did not enjoy reading the present book. Of course I do not know what I should have thought of it fifteen years ago; now it seems to me to be a surprisingly insignificant product of a sparkling mind.

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Here follows a list of corrections to Miss G. E. M. Anscombe's translation.

- p. 17, line 22, read 'again' for 'even'.
- p. 79, line 23, read 'of intersection of significant sentences' for 'to divide significant sentences at'.
- p. 89, line 13, read 'the following point constantly occurs to me' for 'I am constantly struck by the following'.
- p. 153, line 10, read 'definition' for 'explanation'.
- p. 160, line 13 from bottom, read 'something like' for 'as it were'.
- p. 168, line 4 read 'take the metaphysical sting out of it' for 'extract the metaphysical thorn'.
- lines 6 and 5 from bottom, read 'this way of calculating led to the loss of human lives' for 'many human lives had been lost through this way of calculating'.
- p. 169, line 7, read 'cancelling' for 'reduction'.
- line 11 from bottom, read 'destroy' for 'nullify'.
- p. 170, line 10 from bottom, read 'rigour' for 'strictness'.
- p. 174, line 7 from bottom, read 'by-pass it' for 'pass it by'.
- line 5 from bottom read 'continuously and without' for 'without omission or'.
- p. 177, line 5, read 'i.e. we' for 'and hence'.
- p. 183, line 12, read '5' for '50'.
- p. 187, line 10, read 'deranged' for 'not competent to calculate'.

¹ E.g. a purely abstract point of view: J. P. Serre once told me that he saw the mathematical core of the Chinese remainder theorem in a certain result of cohomology theory.

THE SCEPTIC AT BAY

It is good that Professor Ayer has written this book,¹ if only because it is a rounded treatment of a subject in one of the Wittgensteinian ways of handling philosophy. There have been at least two other works of recent times that have also provided rounded treatments of a philosophical problem in a Wittgensteinian way. There are of course differences of outlook, opinion, and method in all of these, but this does not prevent them from being different versions of Wittgensteinianism. Perhaps Professor Ayer does not regard himself as a Wittgensteinian, but this is a matter of classification, and it is legitimate, if we want to, to put together all treatments that are dominated by one of the chief Wittgensteinian trends (provided they are not discordant with other main trends). Even so, he is the least Wittgensteinian of the Wittgensteinians.

Ayer devotes a large proportion of his book to the question of scepticism and certainty. It is made plain that the opponent is always the sceptic, who is constantly challenging any philosophical statement by asking, How do you know? or How can you justify? and every attempt made to answer these challenges is in its turn met by the same challenges all over again. Ayer spends the rest of the work exemplifying this situation. He does so in detail for the problems of perception, memory, and knowledge of others. In the course of doing so, he ranges very widely over a number of other problems such as induction, the thesis of physicalism, and so on; he examines numerous sceptical arguments and counters them with great care; and his writing flows on with his usual attractive rhythm.

His way of grappling with the sceptic's challenge is this. He admits its force. He admits, in fact, its overwhelming force. He admits that there is no straightforward answer to the challenges, that, in fact, no answer that can be given directly to a given challenge is sufficient, because the sceptic is correct in repeating the challenge against each individual attempt at answering the previous one. But, having admitted this, Ayer proceeds to scrutinise the type of argument that is used, or more correctly, the type of demand that the sceptic is making. He contends that the sceptic, rightly unsatisfied with answers that are given him, is pressing for an answer that would be satisfactory, but one that can never be given. And the reason why it can never be given is that, just when the answer has reached the stage of being no longer questionable it becomes a tautology. It is therefore vacuous and cannot serve as a justification for any claim to knowledge. Now Ayer considers that the victory of the sceptic is a hollow one just because the defence against it cannot in principle be fulfilled; and he holds that, because the challenge is one that cannot in principle be met, it cannot properly be made. This thesis Ayer exemplifies in the several topics that he discusses.

¹ A. J. Ayer, *The Problem of Knowledge*, Penguin Books, Harmondsworth, 1956. Pp. 224. 3s. 6d.

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Certain questions arise out of this. Is the conclusion correct that because the sceptic's challenge cannot be met in principle there is nothing really serious about it? Then we may wonder whether the treatment of the subject has a bearing on the philosophy of science. Again it is worth reflecting on a strange twist that this theme has taken historically. Then there is the question of the significance of the problem.

First, the historical position. Perhaps the most famous challenge of scepticism was Hume's attack on metaphysics. This reappeared in the present century in the philosophies of logical analysis, logical positivism, and language analysis. The traditional aim of the sceptical philosophies was to find a criterion by which we could relegate certain sorts of metaphysical assertion, such as those to do with the belief in God or immortality of the soul, to the limbo of things that must be totally rejected, while at the same time allowing commonsense assertions about the existence of objects, about the past, about memory, and so on, to remain and have the status that the man in the street would accord them. Various principles or forms of one principle have been put forward, both by Hume and his modern successors, mainly nowadays some principle of verifiability, which could discriminate between these two classes of assertion. But it has been found impossible to develop a principle that would carry this out, and Professor Ayer's book is, in fact, one of the strange consequences of this lack, for what was found was, that whatever criterion was proposed to rule out metaphysical assertions could, in the end, be turned on the sceptics who used it, so that it ruled out all the ordinary commonsensical assertions that they wished to make as well as those that they wished to disallow. Thus it is the sceptics who are themselves at bay, and there is no doubt an uneasy suspicion that, if any means were found of allowing back into circulation the ordinary statements that are required, there would be no way of keeping out the metaphysical statements that have been so obnoxious.

Another interesting historical light on the subject is this. According to Ayer the only answer to the sceptic would consist of a tautology, which of course would not be satisfactory. But there is clearly another possible answer. What is required is an answer that shall be both unquestionable and non-tautological. Ayer would naturally say this is impossible; but let us ask what such an answer would be like. It is easy to see that such an answer would, in fact, be what was historically known as an *a priori* synthetic proposition. I mention this, not because anyone (whether Wittgensteinian or not) would believe in the possibility of such propositions, but to show that the issue is exactly that between Hume and Kant; for Kant introduced the *a priori* synthetic proposition precisely to justify knowledge in an absolutely unquestionable way and yet without the vacuousness of a tautology. So the issue in quite simple terms is between *a priori* synthetic propositions and scepticism. One can even go further, since the sceptical attack throws

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doubt on the reliability of memory, of induction, of knowledge of objects around us, of other people. It is clear that the consequence of the sceptic's challenge, if successful, is solipsism. Hence the issue is fundamentally between *a priori* synthetic propositions and solipsism.

It is, I think, germane to bring in something of the psychological significance of the problem. Solipsism means 'I alone exist'. Now this implies at least two things. One is that what I take to be the world of memory, of objects, of other people, is entirely of my own making. It is a very omnipotent creation of mine. Secondly, it means that things that happened in the past, other people, and objects around me that are ordinarily taken to exist have been abolished by me, annihilated—a very omnipotent destruction. Thus defeat by the sceptic's argument carries with it the apprehension that I am a being of absolute power carrying out simultaneously an omnipotent act of creation and an omnipotent act of destruction. The sceptical attack was designed in the first case to abolish assertions about the existence of God. That is father killing, and the consequence was found to be that this attack killed off everything else as well and left me to create myself and all my surroundings. This way lies madness.

Perhaps philosophers who make much of this problem in their professional lives are not so deeply concerned about its personal consequences; for it is common enough to read that though solipsism is irrefutable it is unbelievable and we had better disregard it. Still, suppose we take it seriously and consider what it would be like to believe in solipsism. We could not trust our memories, so we should be unsure of the past, and if we did they would be no reliable guide to the future, in the present we should be all alone, and as regards the future we should not know what to expect at all. Anything might happen. We might fail to get anything we wanted and suffer the most frightful privation. We should live in a mood of complete uncertainty, not knowing what to do next and feel thoroughly lost and empty. A situation of more grim despair would be hard to imagine. The uncertainty and emptiness would be felt about everything, but it would be most poignant about the complete absence of any other human being. Disruption of every kind of knowledge would mean not simply intellectual ignorance but the absence of knowledge in the Biblical sense and isolation from any personal contact. Now solipsism reflects an experience that probably is or will be known to most of us, apart from states of acute melancholia or mania, only twice in life—just after birth, and when dying. There can be other moments when we find ourselves alone—even in the presence of another person, as Winnicott and Tranekjaer Rasmussen have pointed out—but most of our other losses, occurring through the death of someone, are less absolute, though sometimes not much less, in that we still have some contact, however inadequate, whether with a 'bus conductor, a colleague, or a tobacconist.

If the emptiness is to be overcome, certainty has to be found. Now when certainty is attained by means of *a priori* synthetic propositions, this means not only that we demand security about our position in the present, *vis-à-vis* other people, some hope for the future, and some reliance on the past, not only security of a reasonably reliable kind but of a kind that is absolutely certain. And this is a demand that would be made only by those who are inordinately apprehensive, in terror of feeling completely empty. (It is at this point that the existentialist philosophy of The Nothing turns out to be so closely allied to logical positivism.) Thus absolute emptiness is countered by a demand for absolute security; solipsism is countered by the *a priori* synthetic proposition.

The psychology of the position, it may be urged, may help us to free ourselves from the bondage of the problem—but the problem surely remains. There is a problem but I wonder if it is so complex. Let us revert to Ayer's conclusion, suggesting that the sceptic's attack is not, after all, so serious. I think that his conclusion is right—down to all but the last clause. There is no reason to conclude that the sceptic's attack can be disregarded. In my view, it finds its target completely; there is no answer at all (so far as the position attacked is concerned): no unquestionable justification for assertions about the past, objects around us, or other persons can be given—there is no certainty at all. I do not agree with the conclusion that the sceptic is demanding an unreasonable form of proof. It is the claimant to knowledge (of a certain kind) who is making an unreasonable claim when pulled up by the sceptic. The situation might be shown thus:

Claimant: The sun rose yesterday, there is the sun now, here is my brother. . . .

Sceptic: How do you know, how do you justify . . . ?

Claimant: I know because . . . , I justify on the grounds . . .

Thus the claimant displays his assumption that he can know or justify (i.e. with certainty). So in asking for the justification, which turns out to be impossible to fulfil because of having to be an *a priori* synthetic proposition (Kant) or a tautology (Ayer), the sceptic is not demanding too much—the claimant has offered too much and the sceptic is only asking whether the offer can be made good. And the claim fails. It is not the sceptic's demand for its substantiation that is at fault but the claimant's contention. If a crank sets out to square a circle with ruler and compass, it is not an empty victory for the sceptical mathematician who shows that it cannot be done. What does the difference come to? If Ayer is right and the sceptic is making an illegitimate demand, the conclusion is simply that knowledge is possible, and Ayer's method does not enable him to add anything further of a positive kind. But we know that already—we know that in some sense or other we have some knowledge. What we want to know is *how* it is possible, what kind of structure knowledge has in order to be possible. Ayer shows

that it cannot be absolute or cannot carry a guarantee. Is that all that can be said? If the sceptic's demand is fair but the claimant is overbidding his hand, we at once have the question, which cannot arise from Ayer's approach, of what sort of thing knowledge is that carries no guarantee.

To consider this, let us return to the sceptic. The sceptic is right: there is no certainty—or guaranteed knowledge—at all. And this is surely the reality. It might happen that all a person's expectations would meet with the most frightful rebuff, that no-one else should exist, and so on. The truth seems to be a simple one: this may happen or it may not happen, and there is no way whatever of knowing which.

Does this mean that there is no claim that can properly be made? Is the only alternative to solipsism the *a priori* synthetic proposition? Surely there is an alternative—to proceed without any certainty at all. And this can be done in the following way. Things may go wrong in every way, as regards our judgments about the past, our perception of things and persons in the present, and our expectations about the future. We have no justification whatever for asserting that things are as we think they are, not even for supposing that they probably are. *But they may be—we do not know that they will not.* And that is all the security we have. Moreover, for the purpose of realising our wants apart from our inner apprehension about it, that is all we objectively need.

To turn briefly to the last of our topics, the philosophy of science. To me the Wittgensteinian methods seems to have no bearing on it. Ayer himself, in the present work, disclaims any intention of doing philosophy of science. He regards his task as purely philosophical, and he makes it explicit that the results can only be tautological and trivial. There is surely something strange in divorcing the problem of knowledge from philosophy of science: scientific knowledge is after all the nearest to being trustworthy that we have (and closely connected with it the historical knowledge of antecedents of change); no scientific knowledge is absolute or certain; in philosophy of science the problem of knowledge is the problem of testing unguaranteeable hypotheses; how then can we reasonably raise the purely philosophical form of the question, to do with guaranteeable knowledge? In the philosophy of science there would be an air of unreality about this question, and a different one would take its place. Hence Ayer would seem to be making an abstraction from scientific knowledge; and, if so, it is hardly surprising that he regards philosophical conclusions as inevitably trivial. Despite my opinion, however, of the lack of bearing of this form of philosophising on the philosophy of science, I have considered it appropriate to review this work at some length in this *Journal* because philosophers of science will wish to estimate the position of the most influential current mode of philosophising.

J. O. WISDOM

La Structure algébrique des théories mathématiques. By Léon Henkin.
Gauthiers-Villars, Paris, and E. Nauwelaerts, Louvain, 1956. Pp. 52.

HERE we have an important and stimulating book which should be read by mathematicians, logicians, and by those scientists and philosophers who realise the complementary rôles of language and algebra in forming the skeletal structure of any scientific theory.

Everybody is familiar with the significance of the group-theoretic viewpoint for the attainment of universally valid laws. One need only mention the invariant theory in geometry, the group of Lorentz transformations in special relativity, the crystallographic groups, and the group-theoretic approach to spin and valency. If we ask why algebra in particular contributes to the understanding of such diverse theories, the answer is simply that it enables us to abstract the essential patterns underlying them and to arrive at meaningful generalisations. On the other hand, we too often neglect the more subtle aspects of language and logical presuppositions in the formulation of our theories.

In the book under review the author discusses with great lucidity the problem of constructing a faithful algebraic model of a class of mathematical theories. He uses Stone's theorem on Boolean algebras to stress the importance of one question: is a given model faithful in the sense that all laws, valid in the systems represented, apply also to the model? Clearly this is belied by the very use of the word 'model'. He then deals with the rôle of language in a mathematical theory and emphasises the inadequacy of the Boolean system. This chapter makes easy reading because formal definitions are relegated to an appendix, which contains also a new proof of the completeness of the language *via* the corresponding theorem for Boolean algebra. However, the main goal of the book is reached in Henkin's treatment of the cylindric algebras of Tarski and Thompson. The author presents his own example of a non-representable algebra and concludes with some conjectures as to the future development of this new type of algebra.

There is a bibliography which the author has wisely and carefully restricted to make it of maximal service to the uninitiated. The monograph is produced by the Tyme-Offset process. The list of errata would be very long; for instance, the symbol of set exponentiation is omitted almost as often as it is inserted. But, fortunately, Henkin's clear and cautiously graded exposition renders most of these typographical blunders trivial.

The author and the publishers ought to be congratulated on having produced a book which provides an excellent introduction to modern researches in a field from which many are turned away by reason of the

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cold austerity of algebraic formalism. It is to be hoped that the monograph will serve the dual purpose of attracting young mathematicians and philosophers of science, and of impressing the relevancy of language in mathematical theory upon those who still consider its study somewhat (*sit venia verbo*)—‘Carrollian’, in the lighter sense of the term.

WOLFGANG YOURGRAU

Science and Human Life : Successes and Limitations. By J. A. V. Butler.
Pergamon Press, London. Pp. 176. 15s.

THE author of this book, J. A. V. Butler, D.Sc., F.R.S., is Professor of Physical Chemistry in the University of London, at the Institute of Cancer Research (Royal Cancer Hospital).

The aim of the book is to examine the question, ‘Can human conduct be based exclusively on scientifically ascertained knowledge?’ The answer which the author gives to this question is ‘No’. He considers that human beings require beliefs which cannot be rationally justified nor receive immediate empirical justification: I say ‘immediate’ because I think the author would contend that the beliefs of which he is speaking, which are of the nature of myths, could be considered justified if there were reason to consider that they had been potent factors in holding a society together for some considerable stretch of time, as, for example, for several centuries: they would, in anthropological terms, be seen to be ‘functionally justified’. He seems clearly of the opinion that contemporary Western society suffers greatly as a consequence of its lack of myths but he gives us no indication of how a modern society deprived of myths could set about acquiring new ones. To expect this would, however, be to ask more than the author promises, for he only intends, I think, to set the limits of what we may expect from empirical science, and to give some indication of what science has so far achieved in its study of man. This is already a sufficiently ambitious programme, and the book ranges over such topics as the method of science, the present achievements of biochemistry, of biology, and of anthropology; there are discussions of our present knowledge of the human brain, and of the relations of brain and mind.

It is clear that such a scope is incompatible with precise scholarship, though not necessarily with interesting contributions to philosophy. Dr Butler, however, does not pretend to be *très calé en philosophie*, and the book is clearly intended for the general reader, and should be judged accordingly.

Judged, then, from the point of view which is appropriate, the book seems to me to be admirable. Too often the general public is introduced to a ‘science’ which is on the eve of solving, if not all human problems, at

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least some of the major problems which have puzzled legislators, moralists, or philosophers, for millennia.

Dr Butler has a firm grasp of the limitations of scientific method in general, and he fully appreciates and clearly explains to the layman how little progress has in fact been made with some of the problems which are in principle amenable to scientific investigation, such as the nature of the mechanism of the brain, or the manner in which genes produce their overt effects.

Personally I have found it intensely interesting to see the way in which a highly competent scientist, who is living, not in the penumbra of the last, but in the light of the present century, struggles to construct some tolerably adequate picture of human nature in the round.

And if Dr Butler's man bears a striking resemblance to Aristotle's, that is something about which I should be the last to complain.

J. S. WILKIE

La Mécanique au XVIIIème siècle (des antécédents scolastiques à la pensée classique).

By René Dugas.

Dunod Editeur, Paris, 1954. Pp. 620. 3,900 Frs.

In this book Dr Dugas, whose magisterial *History of Mechanics* recently made a welcome appearance in an English translation, provides us with a valuable survey, from a somewhat different standpoint, of the mechanical ideas of the great thinkers of the seventeenth century. Unlike his general history, the present book not only concentrates primarily on the underlying philosophical ideas of the subject but is more in the nature of an anthology. It contains much invaluable source material which has not previously been easily obtainable.

The plan of the book is as follows : first a concise survey of previous ideas from Aristotle and Archimedes through the medieval period up to Bruno and Tycho Brahe ; then two short chapters on Kepler and Stevinus of Bruges, followed by longer chapters on Galileo, Mersenne, and Gassendi ; after this a magnificent chapter of over sixty pages on Descartes, a somewhat surprisingly longish chapter on Pascal and another chapter on Cartesian physics (as distinct from the physics of Descartes) ; then we come to the three outstanding figures of the second half of the century Huygens, Newton, and Leibniz, to each of whom a long chapter is devoted ; then a chapter on the theory of external ballistics and the resistance of fluids from Tartaglia and Galileo to John Bernoulli ; finally four general chapters on the interactions and mutual criticisms of the three main schools of thought at the end of the century, Cartesian, Newtonian, and Leibnizian.

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Perhaps the most striking feature of the book is the way in which, by means of carefully selected passages from his writings, including his correspondence, the development of Descartes' ideas on mechanics is presented. Although it must be admitted that, broadly speaking, the judgment of Whewell still holds good, namely that of the fundamental principles of mechanics which were easily obtainable at the beginning of the seventeenth century Descartes grasped as few as was possible for a man of genius, nevertheless one's respect is enhanced for his great pioneer attempt to create a theoretical physics based solely on mechanical laws.

It is a pity that Dr Dugas has confined his attention too exclusively to the general history of ideas, thereby producing a somewhat lop-sided picture. This failure is perhaps excusable in France where there is not such a rigid division as here between science and philosophy, for the French readers of Dugas' book will presumably have some understanding of the technicalities of classical mechanics. But in this country the separation of philosophy of science from science itself is of long standing and any book which, however unintentionally, encourages philosophers to think that they can dismiss the essence of scientific investigation as mere technique and concentrate solely on the 'ideas' cannot be regarded without some regret. Nevertheless, when this has been said, it must be admitted that Dugas has performed an inestimable service for those students who, knowing what mechanics is about, are anxious to understand the tortuous process of how it actually came to be developed.

G. J. WHITROW

Une Tentative d'interprétation causale et non linéaire de la mécanique ondulatoire (la théorie de la double solution). By Louis de Broglie.
Gauthier-Villars, Paris, 1956. Pp. vii+297. 3500 fr.

A WEAK argument does not become stronger by repetition ; nor does it gain in effectiveness by dilution. Professor L. de Broglie, in this new book, has expanded to nearly 300 pages the story, which he has already told us many times in shorter essays and articles, of his pioneering efforts to develop a 'wave mechanics' of atomic phenomena on a classical basis, of his (as we now learn) reluctant adoption of the conceptions of quantum theory, and of his recent return to his old ideas.

The book brings physicists no element which could possibly induce them to alter their considered judgment, passed thirty years ago, about the impossibility of a description of quantal processes in classical terms ; in fact, it has the singular distinction for a physical treatise of not containing a single piece of new experimental evidence which could conceivably bear upon the

problems discussed at such length. The theory of the 'double solution' suffers from a lack of uniqueness in the determination of the non-linear terms of the wave-equation, which puts it on a par with the aether models of the last century.

One would like to say in favour of this book that it assembles in a convenient form all the elements of this 'Scheinproblem', which will have a place of honour in any future 'Budget of Paradoxes'. But Professor de Broglie promises us another book; the theme obviously allows of infinite variations.

L. ROSENFELD

An Introduction to Cybernetics. By W. Ross Ashby.

Chapman & Hall, London, 1956. Pp. ix + 295. Price 36s.

'CYBERNETICS' is a term coined (or rather revived) by Norbert Wiener. His book of this title, which appeared in 1948 was a scientific best-seller of which very few people have read more than the introduction. Wiener's great scientific contribution to the subject was an ingenious mathematical theory of *purposeful* operations on stochastic series. Curiously enough, the major, mathematical part of his book (which very few people have read) had far less influence on contemporary thought than his introduction, which almost everybody has read. Whatever Wiener's intentions may have been, the main effect of his book and of his many lectures in all parts of the world consisted in drawing the attention of biologists, psychologists, and of representatives of other essentially non-mathematical or semi-quantitative sciences to the theory of feedback control systems, which was developed by engineers in the early thirties. (Hazen and Nyquist may be considered as its most important pioneers.) It so happened that soon a wealth of other new ideas started flowing through the propagandistic breach opened by Wiener from the quantitative into the semi-quantitative sciences. Most of these came from Shannon's Statistical Communication Theory, but the general aspects of computer and automaton theory and of operational research soon followed, and in the past years a somewhat heterogeneous compound of all these constituents is sailing under the flag of Cybernetics, at least on the Continent of Europe. (The term is strongly disliked and carefully avoided in British and American 'quantitative' circles.)

It is interesting to see how these general ideas reflect themselves in Dr Ashby's highly original mind. Wiener defined Cybernetics as 'the science of control and communication in the animal and in the machine'. This is general enough (too general for many people's liking), but Ashby makes it even more general by making Cybernetics the science 'not of things but

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of ways of behaving'. In other words it is the functional science of systems in the abstract. Throughout the book the author has done his best to teach his fellow-biologists, to whom this book is mainly addressed, the art of thinking in abstract terms, with the emphasis on function, not on structure. I am unable to make sense of his other definition (on p. 4), that 'Cybernetics might, in fact, be defined as *the study of systems that are open to energy but closed to information and control*—systems that are "information tight"'. Nor can I see that he has taken this definition very seriously.

What the author has done in fact was to give an extensive *dictionary* of terms and concepts which have originated in the quantitative sciences to the non-mathematical scientists. He has not restricted himself to the new sciences (cybernetics and communication theory), but has included the general ideas of classical dynamics, set theory, topology and probability theory, though he has carefully avoided symbolic logic. He has taken great care that his readers shall be 'doers of the word and not hearers only, thus deceiving their own selves' by giving them for every new concept a number of carefully thought out, often ingenious, exercises to solve. This is somewhat unusual in a book addressed to 'workers in physiology, psychology and sociology', and one wonders how successful the author will be with his excellent intentions. It is true that almost any intelligent schoolboy could solve his problems without a great effort, but the adult scientific worker is seldom of the type which delights in solving brain-teasers. It is more likely that most readers will be satisfied with acquiring the vocabulary by reading the text, but this in itself may well be a great benefit to some. An idea of this vocabulary may be gained by the following, somewhat random selection of section headings. 'Transformations, vectors, coupling, feedback, stability, the black box, isomorphism, constraint, the Markov chain, quantity of selection, amplifiers.' One of these, the concept of stability by feedback is probably the greatest gift of engineering to the semi-quantitative sciences. By the work of Tustin and others it is penetrating economics, where its quantitative mastery may well result in progress comparable to that which its intuitive understanding by Keynes and Beveridge has brought about since the dismal thirties. If only somebody with sufficient literary gifts could insinuate it into the heads of politicians!

The reviewer is somewhat doubtful whether Dr Ashby's highly praiseworthy effort will be didactically successful. This book is an introduction to elementary abstract thinking, with only occasional illumination by models. Few people are capable of sustained abstract thought, and these are usually mathematically gifted, there are many more who prefer thinking in terms of models and analogues. Having read this book by the author as well as his previous *Design for a Brain* the reviewer suspects that Dr Ashby himself belongs more naturally to the second type than to the first. The few places where models are introduced, such as the power amplifier on

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page 239, stand out as the purple patches of the book. It might have been better to have more models and less exercises. It is also regrettable that the author does no more than whet the appetite of the reader on the last page with a brief mention of 'intellectual power amplifiers'. If this is kept for a later book, let us look forward to it!

D. GABOR

General Systems: Yearbook of the Society for the Advancement of General Systems Theory, Vol. I 1956. Edited by L. von Bertalanffy and A. Rapaport. Pp. 162. (No publisher or price. Obtainable from the Editors at U.C.L.A., Los Angeles, and University of Michigan, Ann Arbor, respectively.)

SINCE 1930 many have considered that the time is ripe for a morphology of knowledge, covering anything from the fundamentals of physics (Eddington) to all space-time processes (various groups, particularly in the United States, including the above Society). This tendency is now so widespread and includes so many distinguished names that there can be no doubt that it is an expression of the present situation of exact science, implying great potentialities and also considerable dangers.

In view of the pressing need for the unification and clarification of fundamentals the potentialities of this situation require no emphasis, and it is therefore the dangers which call for analysis. Most of the participants in this movement appear in varying degrees to exaggerate their own achievements and to indulge in premature and somewhat indiscriminating formalisation and generalisation. These features render an objective valuation of their work difficult; for example the passage of ten years has been necessary for a widely acceptable critique of Eddington's *Fundamental Theory*. The danger here is not merely that adherents may exaggerate what has been accomplished but that opponents may neglect constructive achievements hidden in a mass of formal analysis. And it may take a long time for both sides to discover their errors.

This dual danger is illustrated in the present volume. The valid aim behind the Society's programmes is the desire to prepare the way for unification. But the present volume reveals that touch of exaggeration which may repel those who might benefit from studying some of its contents.

This yearbook is not a product of the Society's activities, but a collection of fifteen essays, eleven of which have already been published elsewhere up to seven years ago, at least six of the authors being distinguished specialists in various branches of biology. The essays range from Bertalanffy's exposition of his conception of General Systems Theory to Krech on 'Dynamic Systems

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as open neurological systems'. To take one example, it is valuable to learn what M. Calvin has to say on 'Behaviour: Imbalance in a Network of Chemical Transformations'.

The element of exaggeration is occasionally unmistakable. Thus Boulding says: 'General Systems Theory is the skeleton of science, in the sense that it aims to provide a framework. . . .' But a valid 'General Systems Theory' does not yet exist. Moreover those papers which concern themselves with this long-term aim display in some degree one or more of the following failings: undue stress on similarities with relative neglect of differences between one realm and another, premature generalisation, exaggeration of the novel content of the conclusions put forward, or tendency to evade the difficult but crucial task of a critique of the data and of established theories. These weaknesses would not matter so much were the aims less far-reaching, but as it is the average reader may find those passages most satisfying which deal with specific empirical data. This will not be felt when the Newton of a comprehensive theory of complex processes publishes the authentic formulations which so many are now seeking.

But anyone who is genuinely interested in the problems of a unified theory of complex phenomena (in any realm, however narrow or wide) should not neglect those essays which are relevant to his subject matter. Bertalanffy's emphasis on the importance of open systems in biology involved no new idea, but is important, for it leads to still unresolved problems, such as the scope of the different entropy concepts in relation to living systems. If the reader thinks he can discover flaws in some of the essays in this volume, these may be no worse than the often neglected flaws in established theories. Since all scientific theories (with the exception of the theory of the two hundred and thirty crystal classes) are probably imperfect, any fresh attack, such as the one presented here, is to be welcomed.

Unfortunately no attempt is made in this volume to compare and contrast the approach of 'General Systems Theory' with those of various other groups of similar aim. But a fully objective and satisfactory critical assessment of the various potentially universal methods (systems theory, information theory, stochastic theory, behaviour theory, etc.) may only be possible after either physical or biological theory has achieved another fundamental advance. In the meantime all attempts to open up new methods are useful even when they fail, and this reviewer will not condemn any such attempt which is still in an early stage. In this situation each must discover for himself what can help him with his own problems.

L. L. WHYTE

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The Basic Concepts of Mathematics. By Karl Menger.

The Bookstore, Illinois Institute of Technology, Chicago 16, 1957.
Pp. vii + 93.

THIS book can be warmly recommended to all who teach or learn elementary mathematics. Its aims are well set out in the author's Preface:

Algebra, analytic geometry, and large parts of the calculus, as taught today, are products of the 17th century. The fundamental ideas of those branches of mathematics are among the great legacies of that period and have ever since belonged to the most precious heritage of mankind. But with those ideas, the 20th century has also inherited the form in which they were presented by their discoverers. It has inherited the noun *variable* in several meanings that are completely at variance with each other, the noun *constant* in uses that are blatantly self-contradictory; and terms such as *parameter* and *indeterminate* that are utterly obscure. It has further inherited the indiscriminate use of the letter x in more than ten altogether discrepant types of procedure—Renaissance mathematicians practised some kind of x -olatriy. Because all those terms and the symbol x have not been, and are not being, used consistently, their vocabulary and grammar have never been written. This is why those crucial chapters of the theory of the mathematical language have not been, and are not being, taught and why, in the process of learning mathematics, important meanings and rules must be surmised. The deepest difficulties of mathematical education thus are not due to shortcomings in education. They are due to procedures in mathematics.

Although the book is primarily addressed to beginners, it is anticipated that it will also interest the accomplished mathematician and scientist; they will at least be interested in the way the author overcomes the difficulties mentioned above. He himself appeals 'to everyone desirous to clarify his ideas about some of the basic concepts and fundamental procedures of mathematics'.

J. H. WOODGER

Calculus: A Modern Approach. Second, enlarged edition. By Karl Menger.
Illinois Institute of Technology, 1955. Pp. xxiv + 303.

ALTHOUGH the principal object of this book is to bring about a revolution in the *teaching* of the calculus, its interest and significance is philosophical as well as pedagogic because of the author's discussion of the ideas of function and limit.

The author defines the word 'function' and not the usual phrase ' y is a function of x ' which raises the question of the nature of x and y . He defines a function as a class of pairs of real numbers in which two pairs

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which have the same first element have also the same second element. Apart from certain differences in notation from the traditional method, notably dropping brackets and writing fx for $f(x)$, this definition makes it possible to distinguish clearly between the function f itself, which is a class of pairs of numbers, and the value of the function fx at x , which is a number.

The author's new formal definition of a limit is : L is the limit of f at a if fx is arbitrarily close to L for all numbers x sufficiently close to a . This replaces the current terminology 'the limit of $f(x)$ as x tends to a ' which in its turn replaced the phrase 'the limit of $f(x)$ when $x = a$ '. The latter tended to obliterate the important distinction between the limit and the value of the function at a , but the former suggested the idea of 'motion' which is now regarded as irrelevant.

There is much else in the book which challenges the customary notation and exposition of the calculus, both differential and integral, but discussion of these features hardly falls within the purview of this *Journal*. There is no doubt that this is a book of the greatest pedagogic importance. Owing to the marked divergence between the author's methods and those which are now current, it is unlikely that his will be generally followed, at least in the immediate future. Nevertheless, this is a book which should be read by all who teach the subject for it will give them fresh ideas and inspiration.

G. J. WHITROW

The Image. By K. Boulding.

The University of Michigan Press, Ann Arbor, 1956. Pp. 175. 30s.

IN this stimulating and ambitious essay, dictated, as he tells us, in a mood of 'intellectual exaltation', Professor Boulding discusses his concept of the message-image relationship. He shows that the concept of the image may be useful and illuminating in many enquiries, and suggests that the theory of the image, or the theory of knowledge, should be the subject matter of a new science of 'Eiconics', or, at least, an important unifying principle in the sciences. The message-image relationship, as he describes it, is indeed important : whenever an organisation, a human being, an organism, or even a mechanical control system, reacts in response to the stimulus of a 'message', it follows, in Professor Boulding's argument, that the message has changed an 'image', and that the new behaviour is the response of its owner to the changed image. Professor Boulding's subject is the image, its organisation and nature, its growth and change, and its control over behaviour. He discusses the image in organisms as well as in individuals, and devotes several chapters to the rôle of the image in human society.

The personal image is the most complicated. The individual is self-conscious, aware of himself and of his own image. His image includes

his location in time and space, a picture of himself and of the universe, of physical relations, and of his relations with other people. It includes his complicated array of emotions and values. Pictures in the image are held with different degrees of certainty. Parts of the image are only in the sub-conscious. Parts are shared by other people, and are 'public images'. The many parts of the image are acquired in innumerable ways. They are built up from personal experience, from family and formal education. Besides getting information from his senses, man can transmit and receive 'symbolic messages'. He is continually receiving messages, filtering them through his value system, and adding them to his image. His image includes a picture of society, and of his own position in it. The structure and history of society, and the images of individuals, are interdependent. The individual's image of the world, for example, may depend on whether his school atlas was prepared by a German nationalist or by an English imperialist. The nature, growth, and activities of organisations depend on the images of the people who belong to them, and in turn influence the images of both members and onlookers.

How much attention is paid to 'eiconics'? Professor Boulding discusses new work in the natural sciences, information theory, and cybernetics. He suggests that psychology and anthropology are largely 'about images', whereas economics is mechanistic. History is concerned with images (but, curiously enough, historiography is omitted from the list of 'eiconical' studies). This interest in the image is the common ground between the disciplines. How the study of the image is to be a new science, however, is a little obscure. That historiographers, control engineers, psychoanalysts, and geneticists should be aware of their common interests may be a good thing. How a new discipline is to be made of their community of interest is harder to see, while the proposition that it should be depends, of course, on Professor Boulding's view of the rôle of the image.

It is very easy to agree with a good deal of what Professor Boulding says. It is easy, for example, to see as he does the relationship between the image and the disciplines of psychology and anthropology. At times, however, the argument appears to depend on particular uses of the word knowledge. When he says that the carbon atom 'knows how' to unite with four hydrogen atoms or two oxygen atoms, he also says that this is only 'a figure of speech'. The same word, image, is employed in the discussion of the behaviour of the amoeba and the behaviour of the President of the United States. Professor Boulding does not say 'The clock tells the time, therefore the clock must know the time'; but his argument is sometimes rather like that.

The wide scope of this entertaining essay offers Professor Boulding the opportunity for many generalisations. Many of them are shrewd and illuminating, and some, like his comments on the culture of the universities,

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are witty as well. Others, like his observation that poor societies are apt to be more materialist, and richer societies more concerned with 'spiritual affairs', might have been revised. And it is a pity that he sometimes allows himself such expressions as 'soon-to-be-frustrated inside-dopesters'. But the essay should be enjoyed by anyone who is interested in the theory of knowledge and stimulated by speculation.

G. C. ARCHIBALD

Perception and the Representative Design of Psychological Experiments. By Egon Brunswik.

University of California Press, & Cambridge University Press, London, 1956. Pp. vii-xii + 154. 37s. 6d.

BRUNSWIK did not live to see this volume published. This is not an obituary, yet the book I am reviewing summarises the work of this great psychologist.

There is little that has not appeared before, the book being in many ways a re-editing of earlier works, but it is the fullest exposition of his thoughts that there is. Brunswik does not make easy reading, at least not until one has committed to memory and grasped the significance of quite an array of terms. I fear this difficult style (Brunswik originally wrote in German) will be a barrier to the spread of ideas contained in the book which are worthy of the attention of psychologists and philosophers alike.

To give a very simplified account of his position, he made a plea for a type of experimental design in psychology which is more representative, and gives a better sampling of the full richness of ordinary life situations. For this purpose he said we need a 'multivariate' design where, instead of standardising all but one variable as in the classical work in psychophysics and other areas, all the relevant variables are allowed to vary in a random (though measured) way or in a way which 'samples' the actual true life situations. Not only must the stimuli be varied as recommended by R. A. Fisher but also the subjects in the experiment and also, if appropriate, the experimenters. By this means only will the results have any degree of generality.

The other corner-stone of his theory is his emphasis on what he called the functionalistic achievement of the organism in its perception. By this he meant the degree of orderliness and consistency with which it can cope with an environment which is potentially in a state of chaos. Such stability is achieved by what he called the distal focusing of stimulus complexes into 'constancies'. These are the well known constancies of experimental psychology. Brunswik distinguished between the 'proximal' stimuli such as the retinal shape and size of an object and 'distal' stimuli such as

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object constancies. The latter are constructed by a 'probabalistic weighting' of the former and are the stable end products of perception, gained largely through experience. This implied duality of stimulus classes is perhaps unfortunate and unnecessary, it calls to mind the familiar distinction between sense data and objects, and J. J. Gibson's 'retinal' and 'visual' worlds. All that is required is to say that object constancy is the result of a 'confluxion' of stimulus complexes, the constancies themselves do not have independent identity as stimuli. Such a view is consistent with Brunswik's later formulation and he did in fact talk of such 'confluxion' or 'focusing' of stimuli.

His exposition of the rôle of attitude in psychological experiments in chapter XIV should be read by all who are interested in perception. Lack of clarity in the definition of the task asked of the subject in the experimental situation has caused widespread confusion in the literature. Brunswik's writings on this matter are most refreshing and helpful.

In the second part of the book Brunswik turned from a basic exposition of his theoretical position to review experimental work which he considered 'representative'. Some of this work, especially the experiments on weight illusions, I find in no way different to classical experiments. The experiment on the judgment of faces, while admittedly representative, seems trivial and uninspired to me. Many well known experiments and procedures which are not mentioned are, as I see it, well qualified to be labelled both representative and functionally orientated. I am thinking of time sampling studies of child play, Piaget's study of children's thought processes and sensori-motor habits, the War Office selection procedures for officers during the war, and much of modern sociology and anthropology. Such methods are used where appropriate, and without fuss. Brunswik's critical remarks are, however, probably warranted within the academic study of perception and learning, where there has been too often a narrow conception of experimental design. Here Brunswik's ideas are like a breath of fresh air.

It would be a mistake to suppose that the older methods are now useless and superseded by representative design throughout the whole domain of psychology. Brunswik did not claim this, indeed he stressed that for certain problems the classical, univariate, controlled experiment is appropriate, as long as the legitimate limits of generality of the results are not exceeded. An overdue emphasis on representativeness may have its own dangers, for pushed to its logical conclusion the ideal experiment would be everyone's and anyone's observations on everyday life, but even that may be a good antidote for learning theorists emerging from a generation of pre-occupation with rats in mazes, and dogmatic assertions about the rôle of the hypothetico-deductive method in psychology. We owe a lot to the late Egon Brunswik.

I. P. HOWARD

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Problems of Consciousness : Transactions of the Fifth Conference, 1954.
Edited by Harold A. Abramson, M.D.
Josiah Macy, Jr. Foundation, New York, 1955. Pp. 180 \$3.50.

THIS is another of the volumes, by now becoming well known, in which a number of eminent contributors open the ball with papers and have their subsequent discussions taken down in shorthand. Some of the volumes produced in various fields have been remarkably good considering their impromptu nature. The conferences were obviously worth holding and the volumes are of some value as a stimulus.

But it cannot be denied that some of the volumes have grave defects. In a previous review in this *Journal* on previous transactions about consciousness I drew attention to the fact that the participants had no definite problem before them. The present volume, the last of the series, says that they have discovered this. The subject peters out without their ever being able to find a topic and pull the threads together. It is perhaps the weakest of all the volumes.

On page 93 we read, 'I do not know what we are arguing about.' This was the first sign of good sense.

On page 131 we find the extremely illuminating information that a correlation has been found between birth with long labour and failure to keep appointments.

On page 134 we are reminded of the need expressed at the previous conference to have a methodologist. This is just conceivably an understatement.

On page 147 an attempt is made to define a science : 'that which lends predictability to phenomena is scientific.' One is not impressed to find pronouncements like this still being made in serious discussions by first class savants.

On page 160 in the closing remarks we find a penetrating estimate of the position reached : 'although I still do not know what anybody else means when he says "consciousness", I have a much better understanding as to what I mean by it, even though I cannot put it into words.' In short the conference can hardly be said to have got very far ; and it is indeed doubtful whether the present volume can truly be said to be about anything.

J. O. WISDOM

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Philosophy of Science, 1957, 24, No. 1

W. P. Alston, 'Is a sense Datum Really Necessary ?'

It has been claimed that certain perceptual situations (e.g. complete hallucinations) can only be described in terms of sense-data. But careful survey of ordinary language reveals a large number of expressions, e.g. 'It appears that—', 'It seems that—', 'It was as if—', etc., which contain no phrases referring to sense-data, and which can be used for such descriptions. Such sentences are sometimes used to indicate degrees of probability, but they also have the phenomenological use. Moreover such sentences bring out the similarity between normal perceptions and hallucinations at least as well as sense-data sentences.

Philosophy of Science, 1957, 24, No. 2

C. Frankel, 'Explanation and Interpretation in History'

This paper analyses certain key issues in the logic of historical explanation with a view to determining the relation of 'interpretation' to 'explanation' in history. It is believed that historical explanation, although it has certain distinctive features and is frequently dependent on views about the 'meaning' or 'value' of given events, is nevertheless continuous with the logic of disciplined inquiry in other domains. An analysis of types of historical interpretation is offered in an attempt to show that arguments about the proper interpretation of historical sequences are usually genuine arguments rather than pseudo-arguments, and that they are at least in principle capable of being resolved by appeal to objective considerations.

Philosophy of Science, 1957, 24, No. 3

H. Hay, 'Free-will and Possibilities'

The paper discusses whether the believer in free-will had hold of any points omitted by determinists but not inconsistent with determinism. An examination of William James's 'Dilemma of Determinism' showed that he rejected determinism because it denied the existence of alternative possibilities and because without these regret would be unreasonable. However, James's attack was against what can only be regarded as one species of determinism, a monistic holism. Determinism in the broad sense allows us to define what is meant by the existence of possibilities. Possibility is a relative term. A possibility exists with reference to a specific situation and is what the description of that situation together with true generalisations implies when certain additional particular facts outside the situation are added. Hence, with reference to a given situation there are alternative possible outcomes, with reference to different additional particular facts.

H. Hiz, 'Types and environments'

The notion of type (like style in art, period of history, phoneme) can be analysed by environments. For this purpose one may take a restricted concatenation that

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allows one to juxtapose only such segments that actually occur one after the other. Two segments may belong to the same type if their environments are always different.

H. A. Patin, 'Pragmatism, Intuitionism, and Formalism'

This paper deals with a quandary which engaged Peirce as to the result of applying his pragmatic maxim to the concept of 'incommensurable'. Peirce's views are examined and it is then shown that later pragmatists, especially Dewey, by profoundly enlarging the notion of 'practical bearings' are liable to give a satisfactory account of abstract concepts and systems which is clearly in the spirit of Peirce's original intent, and in accord with his conception of mathematics. The solution that Peirce summarily discarded and the one that he finally offered are seen to be closely related to the mathematical schools of 'formalism' and 'intuitionism' respectively. The thesis is then advanced that although the latter has an approach strikingly similar to Peirce's last 'operational' formulation, it is actually 'formalism' whose intent and method lie within the present scope of pragmatism.

R. R. Delgado 'A possible Model For Ideas'

The article presents 'the Idea' as the differentiating characteristic between man and animal. Ideas constitute the basic tool of man. Comprehension of the structure of ideas would be useful to understand the function of different and often contradictory ideologies. This article considers the element of ideas to be: (1) Perceptible worlds, specific, individual and social. The social perceptible world is divided into linguistic, objective and professional subspheres. (2) The intuitive world, in which is described a scale of growing intuitive abstraction, differentiated into mystic and aesthetic experiences. (3) The conceptual world, hypothetically evolved through four main stages: perceptive, intuitive, conceptual and experimental. In the experimental class appear the actual forms of relativised, synthetic and integrated concepts. A division between closed and open concepts is offered, the open concept being dynamic, without absolute borderlines, forming a structural continuum and requiring for its treatment a new kind of Logic. (4) The affective field, which is reduced to a minimum by logical symbolism and mathematical forms of expression. These four intellectual mechanisms or processes are in an 'idea of the idea', an operational symbol that perhaps may be used, from a unified point of view, in psychology, logic, gnoseology and philosophy. A graphic model of the idea is proposed, and a possible classification of its individual and social types.

L. Reiser, 'Matter, Anti-matter, and Cosmic Symmetry'

The present article accepts a postulated cosmic symmetry, but holds that 'anti-matter' requires a complementarity between 'particles' and archetypal (organising) forms in the Cosmic Field of Energy. The existence of anti-matter (e.g. neutrons and anti-neutrons) no more requires the existence of 'anti-galaxies' . . . wrongly supposed to be receding from each other since the 'big bang' five billion years ago . . . than the existence of organisms requires the existence of anti-organisms.

The writer's *cyclic-creative cosmology*, based on an *energy matter* reciprocity and the continuous creation of hydrogen atoms, builds on the hypothesis that positive and negative pair-particles (electrons and positrons, protons and anti-protons) may represent

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appositely modified spiraloids in the Cosmic Field, due to differences in rotations associated with operators capable of double rotations in the hyperdimensional Field, thus creating enantiomorphic (mirror-image) forms. The relation of this conception of matter as spiraloids of energy to the recently announced Yang-Lee dissymmetry of spin-particles ('fall of parity') still remains to be explored.

Philosophy of Science, 1957, 24, No. 4

H. Marganau and R. A. Mould, 'Relativity: An Epistemological Appraisal'

This paper is the forerunner of an extensive logical analysis of the relativity idea, in which an axiomatic structure based upon the principles of topology is developed. It is meant to expose the manner in which relativity stretches from the pole of pure conception to that of factual observation, from the *a priori* to the *a posteriori*. In connection with special relativity, it is shown precisely which elements are postulational and which are verifiable empirically. The authors attempt to show how misguided it is to 'derive' relativity from such experimental facts as the Michelson-Morley experiment—as so many textbooks profess to do.

A. Landé, 'Non-Quantal Foundations of Quantum Theory'

The article aims at a better understanding of the quantum theory by reducing its principles and mathematical rules to elementary postulates of an almost common-sense non-quantal character. In particular, the strange laws of the probability amplitude Ψ as well as the periodic relation between energy and time, momentum and position are developed as consequence of simple ground postulates: continuity of cause-effect relations (implying necessarily the existence of acausal discontinuous events), reproducibility of a measurement, the postulate that there be a general structural metric of the probabilities at all, and constant statistical density in position-momentum space (taken from classical statistical mechanics). Combination of these elementary postulates leads to Planck's and de Broglie's equations and all the rest. In contrast to the current opinion, propagated by the Copenhagen School, that the 'quantum principles' of complementarity, etc., represent the ultimate bottom of theoretical analysis, the article shows that there is a more elementary background upon which the quantum theory including its physical 'principles' and mathematical prescriptions can be erected.

E. Grunberg, 'Notes on the Verifiability of Economic Laws'

The weakness of the explanatory powers of economics is due to the concern of economics with unique events where this term is taken in the sense of: all that goes on in a specified spacetime region. The explanation of unique events is always incomplete and has therefore been called an explanation-sketch by C. G. Hempel. Since such a sketch refers explicitly to a non-repetitive event it cannot be verified in the customary way by prediction. Therefore general laws must be verified independently from their use in explanation-sketches. General economic laws cannot be disconfirmed by incorrect prediction since a discrepancy between predicted and observed event may always be explained by the nonfulfilment of the *ceteris paribus* condition.

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If a law cannot be directly or indirectly verified, it may be possible to deduce it validly from true premisses. This apparently is the procedure followed in the deduction of economic laws from the rational principle. The trouble with this is that the rational principle itself can be verified only by verifying the theorems deduced from it. Thus the justification of criticism directed against the rational principle lies not in the fact that human beings do not always act rationally, but in the impossibility of strictly disconfirming the theorems deduced from it.

Since economics deals with certain aspects of human behaviour, it seems promising to deduce economic laws in which no behavioural terms appear in reference to the *explicandum* from laws referring explicitly to regularities in human behaviour.

While the suggested reduction permits satisfaction of the requirement that all laws entering explanation-sketches must be verified independently from such use, it leaves intact the difficulties arising from the incompleteness of the explanation-sketch. This difficulty alone, however, is not insuperable.

J. P. McKinney, 'Knowledge and Experience (Comment on a paper by L. von Bertalanffy: "The Relativity of Categories")'

The 'relativist' view of knowledge is an expression of the modern rejection of the 'classical' view that knowledge is an objective report on an 'absolute' world. The relativists (von Bertalanffy *et al.*) see knowledge as necessarily bearing the stigma of the experiential (biological and cultural) conditions of which it is an expression. The individual experiences in terms of the established body of categories, or what may be called the common world-picture. It is pointed out that the relativist standpoint, as an outcome and expression of the tracing of the structure of knowledge back to its basis in experience, conflicts with the view that an 'absolute' basis of knowledge is being revealed by the elimination of all elements of experience from the structure of knowledge. On the contrary, the implication of the relativist standpoint appears to be that, if an absolute core is being revealed by the reduction of knowledge to its basis in experience, *then* this absolute would have to be sought in experience itself. The view (Whorf) that the relativist standpoint should lead to a revaluation of established categories, is itself an outcome of such a revaluation; and the view (von Uexkull) that human knowledge 'is in no way singular as compared with that of the sea-urchin, the fly or the dog', overlooks the fact that man is the only creature to first build up a body of *objective* knowledge from the data of *subjective* individual experience, and then submit this body of knowledge to systematic analysis, in which its principles of organisation are brought up into consciousness as a new and more powerful kind of knowledge.

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THE ANNUAL CONFERENCE OF THE PHILOSOPHY OF SCIENCE GROUP

THE second annual conference of the Philosophy of Science Group of the British Society for the History of Science was held from the 20th to the 22nd September 1957 at Wortley Hall, University of Nottingham, under the Chairmanship of Professor R. O. Kapp, chairman of the Group, and was attended by about fifty members and guests. Dr E. H. Hutten was conference secretary and Dr M. B. Hesse acted as conference treasurer.

Symposia were held on 'The Logic of Probability', 'Observation and Experiment', 'Homeostasis' and 'The Mind-Body Problem'. In the first symposium papers were read by Dr J. Bronowski and Dr W. Mays and Mr W. C. Kneale took the chair. Dr Bronowski discussed different senses of probability. In the main sense that he discussed probability is concerned with laws and *not* with isolated events: it is inference probability. There is no meaning to the question 'what is the probability of the sun rising tomorrow?' Perhaps it would be better to drop the term 'probability' in the context of induction and to say that a law is reasonable rather than probable. However, the establishment of a law is not like drawing a sample from the universe nor is it done by repetition only. Both procedures are not scientific induction. What we do is to estimate that the numerical parameter of a law lies within certain limits. The null-hypothesis is simply a law in which the parameter is put at zero value and so we test whether the parameter significantly deviates from this value. Fisher restricts the parameter to the interval between certain confidence limits and assumes an *a priori* probability distribution for it. Dr Bronowski explained that *in fact* we never know such a distribution. Neyman and Pearson thought that if we can establish an hypothesis with a probability of 95 per cent then we could be certain to the same extent of what the probability distribution is within a confidence interval. This is not true; for significant tests do not represent a probability distribution, i.e. to have 95 per cent confidence (or to be only 5 per cent 'out') is not the same as to score 95 per cent. success in repetition. It is in the nature of things that you cannot assign a number to an inductive inference. The choice between one or other new law is based on whether its coherence with established laws is better or not. The recent experiment regarding parity and nuclear reactions is an example. We have a prejudice in favour of symmetry; therefore we are astonished when the parity law is broken. Its coherence with the laws of quantum mechanics is different from what we assumed naively. Relativity Theory is not a correction to Newtonian mechanics but a new organisation of experience in a different pattern of coherence. You cannot compare two different organisations of this kind and assign a number to each of them. Dr Bronowski suggested that a coherence estimate may possibly be made by means of a topological measure. Of two theories, that one is preferred which is most closely integrated. Thus general relativity is better than Newton's theory, for it can not only describe all the phenomena that classical theory can, but over and above this it establishes new relationships between them. Dr Mays discussed the pragmatics of probability and described the work of A. Naess on the experimental semantics of probability. Subjective probability was found to have the same

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properties as objective probability. The calculus of probability can be regarded as a model of the maturity of chances. Chances have to be learnt and the work of Piaget shows that the search for regularity is abandoned in children as they mature, probability judgments then being substituted. A lively discussion followed these two papers.

The second symposium took place on Saturday morning with Professor H. Dingle in the Chair. Dr Mary Hesse criticised the 'dictionary theory' of some philosophers of science on the ground that if the meaning of a theory depends solely on its translation into phenomenal statements whose meaning is independent of the theory, the theory can never be tested. Mr P. Alexander defended the 'dictionary theory' by maintaining that the meaning of phenomenal statements is independent of the theory, for the theory explains them, and would not do so if it were pre-supposed by them. Mr R. F. J. Withers discussed the relation between observation and experiments in certain problems in biology. In the Golgi controversy much argument is generated by workers thinking that observation alone will give them information about the structure and possible function of the Golgi apparatus; in other words they assumed that observation alone enables them to choose between different interpretations of the phenomena. Yet in the absence of a theory concerning the functioning of the Golgi apparatus no experimental work is carried out and no interpretive choice can be made. The controversy was discussed in the light of the classical epistemology of 'sensible' and 'physical' objects. By means of these terms Mr Withers maintained that sense could be made of the controversy over what is *artifactual*. During the discussion there was some clarification of levels of observation statements: theories are not explicitly reducible to observation and the *truth* of the theory which explains them must not be presupposed, although some lower level theories may be, and the *language* of the explanatory theory must be.

Professor R. O. Kapp took the chair in the Saturday evening session. Professor A. C. Mace criticised Freud's homeostatic concept of biological systems as those which tend to abolish stimuli and wished to distinguish between genuine and goal-directed systems and those which are satisfactorily explained by causal laws. Dr N. Walker exposed the inadequacies of current ideas of homeostasis by pointing out that they are too all-embracing. For example no distinction is made between static and dynamic homeostasis. A better term to use here might be 'isonomy'. The popular notion of homeostasis has become something like a final cause. This is due to the idea that the central nervous system has the sole function of abolishing stimuli or that there is only one kind of stimulus, namely the stimulus of abolishing itself. Homeostatic systems are very different from what we normally call a 'system'. We have already a complicated system when we take a pencil and try to place it on its end on the table. Adjustment as a homeostatic process may require us to envisage end states that cannot be observed. Such states may only be such that they last longer than any other, for example, the egg may be a final state rather than the hen. There is a set of covarying variables. At least some of them vary according to a certain pattern. The roulette wheel tends to maintain relative frequency but it still leaves us with a desire to find out the exact mechanism of the homeostasis. Dr Walker believed the more precise definition of homeostasis is needed. The homeostatic system may involve negative feed-back, damping, underdamping, coarseness of response, communication, amplification, reaction time, etc. This criteria was mentioned in the discussion that followed.

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On Sunday morning the symposium on the Mind-Body problem took place and its chairman was Dr E. H. Hutten. Professor Woodger suggested that an approach to the mind-body problem might be made by considering the sentences of the form *x is getting y of z*. This functor was a three-term relation whose first domain (D_1) consists of persons, its second domain (D_2) consists of sensible objects and its third domain (D_3) of physical objects. Professor Woodger implied that much of the classical discussion of the Mind-Body problem could take place with greater clarity by using this functor. He pointed out that physical science dealt mainly with statements in D_3 and he felt that there was an urgent need to examine the relationships between statements in D_1 and D_3 particularly in the medical sciences. Mr C. K. Grant made a detailed analysis of the various philosophical assumptions involved in Professor Woodger's approach. He discussed the relationship between public and private knowledge and whether D_1 , D_2 , and D_3 were as independent as Professor Woodger thought. The discussion showed that Professor Woodger's approach was novel and many speakers argued about the possible entities that might be put into the various domains of his functor.

The conference closed with a general discussion during which the hope was expressed that a third annual conference would be held during September 1958 and possible subjects for this were suggested.

ANNOUNCEMENTS

PHILOSOPHY OF SCIENCE GROUP CONFERENCE

The Third Annual Conference will be held at St Hilda's College, Oxford from 19th to 21st September 1958. Among the speakers will be Professor T.W. Hutchison, Dr G. Buchdahl, Mr E. Gellner, and Dr H. R. Post-Paneth. The Conference fee will be £3 12s. for full residence. Further details and registration forms can be obtained from Miss M. B. Hesse, University College, London, W.C.1.

DAEDALUS

The American Academy of Arts and Sciences have launched a new quarterly publication called *Daedalus*. The first number contains articles on science and the modern world view, of interest to our readers. These articles will be found listed among publications in the Philosophy of Science. We wish the new Journal every success in the future.